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VOL. XVII No. 68 NOVEMBER 1960

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Published six times a year by The British Association for the Advancement of Science
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Subscription Rate: Per annum 40s. *post free* ($6). *Single copies* 7s. 6d. *plus postage* 7d. ($1.25 *post free*)

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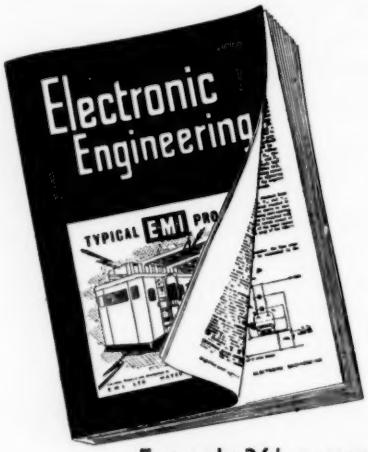

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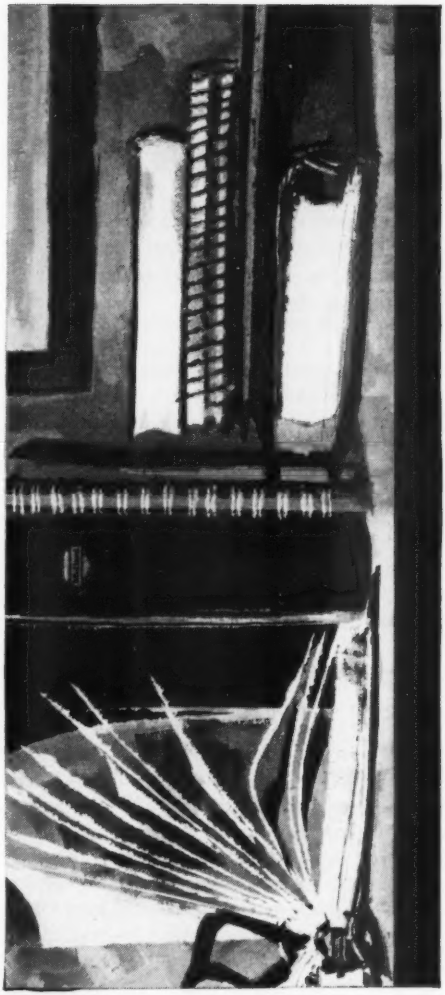

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Notes and Comments

Science in the Advancement of the New States

Every night, as darkness closed the sessions of the conference on *Science in the Advancement of the New States* held at the Weizmann Institute, Rehovoth, Israel, a bright star moved across the cloudless sky. The politicians and civil servants of the thirty-one Afro-Asian states participating would watch the U.S. artificial satellite crossing from horizon to horizon in fifteen minutes. Then they would resume their conversations about the tsetse fly, illiteracy, infantile mortality and the problems of food and population. The transit of ECHO was symbolic—the capacity of science to master nature and adventure into Space while, for two-thirds of the world's population, the problems of bare survival are still unsolved.

That was the salutary lesson of this unusual conference which Mr. Abba Eban, in his position as President of the Weizmann Institute of Science, had called. The intention was to introduce the 'New Men', leaders of the emergent countries, to eminent scientists from the advanced countries so that they would know how science and technology might help them with their problems. Nevertheless it is fair to say that, in the end, the Teachers were the Taught and it was the scientists who learned the facts of life from the 'New Men'.

Sir John Cockcroft opened the scientific sessions with a survey of modern science—from the nature of the nucleus to the edge of the expanding universe—which was a *tour de force*. Then Prof. P. M. S. Blackett, following a cautionary survey of the economic needs and potentialities of underdeveloped countries by Prof. W. Arthur Lewis, warned them of the seductions of 'The Supermarket of Science'. He advised them to leave advanced science to the countries which could afford it and to train their people meantime to recognise, and to use, the 'know-how' which was already available for most of their needs. They ought to prepare their shopping-lists carefully otherwise they would find themselves with the luxuries of science and without the necessities. Mr. John Jukes, of the British Atomic Energy Authority, gave them convincing arguments why atomic-reactors would not be their short-cut to industrialisation, while assuring them that when they were ready to use it, atomic energy would be abundantly available to them. Dr. Jules M. Gueron, of EURATOM, used that consortium of advanced European nations to show what a costly and problematical venture nuclear development is. Dr. Alvin M. Weinberg, Director of Oak Ridge National Laboratory, Tennessee, told them how breeder reactors would enable them 'to burn their own rocks' and get energy self-sufficiency—twenty or forty years from now! All this was intended to discourage them from indulging in atomic energy, which Dr. Brock Chisholm, former Director-General of WHO told them was 'the new magic'.

When, however, the 'New Men' answered back, it was clear that they had no illusions about the 'new magic'. The first to jolt the conference back to stark realities was the Rev. Solomon Caulker, vice-principal of Fourah Bay University, Sierra Leone (who was tragically killed in the crash of the aircraft which was taking him back from the conference). Mr. Caulker said he was not interested in 'prestige projects'. He wanted to know what science's answer was to his witch-doctors and how it could persuade his people that the eight out of ten infants who died were not bewitched and that the demons were the germs in the dirty water. From there on, the conference got down to the immediacies—education, better food yields, pest-control, disease-control, harvesting the seas (Mr. Michael W. Perrin) and more education. It was the

'New Men' who suggested regional instead of national research stations and who wanted to safeguard their talented students who were liable, on fellowships, to be retained by the advanced countries. That was why they wanted well-equipped centres, co-operatively provided, to which they could return and help to train more students.

The real achievement of the conference was not in the Wix Auditorium symposia but in the cordial, off-stage, relationships established between the scientists and the leaders of the New States. The outcome was the Rehovoth Declaration, which the chanceries of the world would do well to notice. It established what might be called a 'Science Consumers' Co-operative', well-based on mutual, and scientific, assistance.

RITCHIE CALDER

Food enrichment as a national policy in South Africa

Food enrichment is a subject which has been given much consideration as a means of making good deficiencies or imbalance that may occur in national food supplies. Its simplicity may, however, belie its applicability. It was with a view to making it part of the national food policy in South Africa that bread should be enriched with protein as fish flour that a number of organisations co-operated from 1956 onwards. The results of their studies were published in 1959 and, as the principles underlying the whole subject of food enrichment were considered, the report will be of the greatest interest to other governments which may contemplate food enrichment on a large scale.

It brings together the information available on the occurrence of protein malnutrition in South Africa and gives the results of the extensive amount of work undertaken on the technology of production and the nutritive value of fish flours prepared from the pilchard *Sardinops ocellata* and maasbanker *Trachurus trachurus*.

While the evidence indicated that the fish flour which had been produced was a source of protein suitable for human consumption, the weight of that evidence was not enough to warrant a recommendation that the scheme for its use for fortification of bread should be adopted on a national scale in the Union. For the non-European infants and young children, among whom kwashiorkor is widespread, skimmed-milk remains the diet treatment of choice.

D. P. CUTHBERTSON

Association Française pour l'Avancement des Sciences

The A.F.A.S. held its annual meeting for 1960 in Grenoble the attractive 'capitale des Alpes Française'. To the writer it would appear that the choice of town had a significance relevant to some of the current national problems of France.

Grenoble is a recognised industrial centre of considerable importance. France's hydro-electric and hydraulic industries are located here and France is concerned now with the need to encourage industrial decentralisation. Added to this, the University of Grenoble appears to be one of those that are challenging the over-concentration in Paris of French higher education. Thus, both as a centre of industry and as a seat of learning, Grenoble seems to represent the trend that is challenging the supreme status of Paris.

Like the British Association, the Annual Congress of A.F.A.S. is essentially based on Sections which arrange their own scientific programmes. Unlike the B.A. the Sections include a number of the medical sciences—such as hygiene and public health, dentistry, pharmacology and 'medical science' itself. Again in contrast to the B.A. a number of the Sections group together for much of their activity; thus one group is made up of the following Sections—botany, 'Biogeography' (ecology), zoology, general biology, agriculture and forestry. Papers are arranged by the groups so that any particular Section might provide few or even no papers at an Annual Congress. In this Congress, the Sections providing the longest programmes were jointly, pharmacology, biochemistry and biology, and separately, dentistry.

Another difference between the B.A. and the A.F.A.S. is simply in terms of size. Whereas B.A. meetings have, in recent years, numbered between 2,900 and 4,000 members, the number of participants at Grenoble was about 250. This naturally affects the whole character of the meeting. The Presidential address, for example was comfortably housed in a large lecture theatre of the Technical Institute. Individual Section meetings took place

in the classrooms of a beautiful new Lycée; frequently the size of the audience was such as to change the nature of the proceedings from the formal presentation of a paper to an intimate discussion between colleagues. It is clear that such an exchange of ideas in this informal manner and in this friendly setting can be of immense value to those taking part. But equally clearly the 'intelligent lay public' was not having the access to recent scientific work in the way that this occurs at the B.A. meetings.

So far as could be judged from various delegates, from the titles of the papers and from discussions, there was a strong tendency for papers to have a considerable practical bias. A.F.A.S. does not appear to be the platform for pure science so much as for its application.

In addition to the programme of papers, there were scientific and 'popular' visits arranged for the delegates, and from this point of view, Grenoble is an ideal centre. A particularly interesting visit was to the new transistor factory of the great C.S.F. company. This factory has recently been built in a mountain setting at Saint-Egrève, a few miles from Grenoble. Locating it here appears to be part of this company's contribution to the decentralisation trend already mentioned. The particular interest of the factory was equally in its original and clever planning and design as in the up-to-date processes that were seen. Built on a steel frame, it consists of a series of long narrow single-storey bays somewhat like a line of shining aluminium railway coaches in their proportions. This design lends itself to considerable flexibility, the 'skin' walls being prefabricated panels which are easily removed to admit (for example) new plant or equipment. A highly imaginative colour and lighting scheme in each block, carefully planned air conditioning and insulation of the walls, together with an attention to cleanliness that exceeds anything that the writer has seen, made the workrooms of outstanding attraction. This is certainly a factory which can have few equals in terms of its general environmental conditions.

Another visit, limited to a small number of fortunate people, which deserves mention was to the Mont Blanc Tunnel. This was not only of considerable geological interest but is clearly an engineering feat of considerable importance. Furthermore, the economic implications of this project are of the utmost importance to France and Italy. It would certainly seem that this project has caught the imagination of France in much the same way as the building of our M1 appealed to the British public. When finished, the tunnel will be some 12 kilometres in length and will greatly ease the road traffic from France to Italy.

In conclusion, it seems evident that the two Associations for the Advancement of Science have more features in common than they have differences. Between the two, there exists a friendship which is of the greatest value to both.

H. G. MAULE.

The National Book League and Science

Never has it been so important that the layman should have some knowledge of the scientific world. One obvious way for him to acquire it is through reading, and the National Book League* does a great deal to see that he finds the right books.

One of the League's principal methods of doing this is through its exhibitions. The SCIENCE FOR ALL exhibition contains about 400 books on the physical and biological sciences for the non-specialist. It is revised each August, and the new edition launched at the Annual Meeting of the British Association for the Advancement of Science. At the same time an annotated list (price 1*s.* 6*d.*) of the twelve months' new publications is issued, supplementing the main National Book League bibliography *Science for All* (C.U.P. for N.B.L., 9*s.* 6*d.*, members 7*s.* 6*d.*). In future, new books to be included in the next year's supplement will be serially reported in *The Advancement of Science*. 100 BOOKS ON SCIENCE TODAY is a smaller exhibition, and is a selection of books intended to give a quick survey of present-day sciences. These two exhibitions are hired by Education Authorities, who show them centrally for the benefit of teachers and sixth-formers; they are also shown in bookshops, and public libraries.

Other N.B.L. science exhibitions are mainly directed to the schools. On the current list are SCIENCE: 50 BOOKS FOR SECONDARY SCHOOLS and NATURAL HISTORY: 50 BOOKS FOR SECONDARY SCHOOLS. These consist of recent books on their

* 7 Albemarle St., W.1.

subjects for boys and girls from 11 to 15, and are hired by schools and Education Authorities. The SCHOOL LIBRARY EXHIBITION which, in all, contains 2500 books, includes large sections on science, natural history, and applied science and technology.

Two new touring exhibitions are of books for teachers, excluding text-books. They are: THE TEACHING OF SCIENCE, books selected by Mr. M. Underwood, B.Sc., Lecturer in Science at the College of St. Mark and St. John, Chelsea, and THE TEACHING OF MATHEMATICS, books selected by Mr. N. Payne, Lecturer in Mathematics at the Borough Road College, Isleworth. They have recently been shown in the League's gallery at 7 Albemarle Street, and start their touring lives this autumn. The National Book League keeps in close touch with the Science Teachers' Association, and is working out methods of cooperation.

Sir Charles Snow, in his *The Two Cultures and the Scientific Revolution* (C.U.P., 1959, 3*s.* 6*d.*), deplored the unbridged gulf between science and the humanities. Apart from bringing science to the non-specialist, the N.B.L. attempts to reverse the procedure and interest the scientist in subjects that may be unfamiliar to him. For instance, at the B.A. Annual Meeting in Cardiff (August 31 to September 7), three exhibitions, as well as SCIENCE FOR ALL, were shown with this end in view—ECONOMICS, EDUCATION and SOCIAL STUDIES. Another exhibition which might appeal to the scientist who wishes to broaden his reading is TWENTIETH-CENTURY ENGLISH READING: A GUIDE TO MODERN ENGLISH LITERATURE. The bibliography on which this is based—*Signposts*—can be obtained from the National Book League (price 5*s.*, 3*s.* to members).

The League's Book Information Bureau, which answers hundreds of questions a week on all types of books, will give information about scientific books and prepare book lists on scientific subjects—often obtaining help from specialist sources.

The N.B.L. works, whenever it can, with industry, and it is, for instance, providing book exhibitions at a managers' course to be run by one of Britain's largest industrial organisations. Those attending the course will see TODAY AND TOMORROW, which gives a broad picture of the complexity of modern life and thought in all fields of knowledge; SOCIAL STUDIES, and BUSINESS ARCHIVES.

It will be seen that the National Book League hopes to provide not only books and information for the man interested in science, but also books for the scientific man interested in thought and ideas of all kinds.

Corrosion

Corrosion in the United Kingdom is costing about £600 million pounds every year and even a slight improvement in preventive measures would represent considerable savings in materials and man-power.

The National Chemical Laboratory, in its recently published annual report, outlines ways in which it is tackling this urgent national problem: it is extending its work on bacterial corrosion by research on factors affecting the aggressiveness of soils; making a series of tests on the corrosion of mild steel ship's plates in estuary water; an investigation of the corrosion of mild steel tubes in 'Scotch' marine boilers has been completed for the British Shipbuilding Research Association and a pilot survey is being carried out on corrosion by polluted waters.

Research in this field is not confined to fundamental work, though, of course, to understand some of the problems of corrosion further basic knowledge is essential. Much applied work is also being done, especially in co-operation with industry.

D.S.I.R. Report

Frost-resistant Seeds for Soviet Market Gardeners

A method of treating vegetable seeds so that they will withstand extreme winter cold has been reported by scientists working at the Timiriazev Agricultural Academy in the Soviet Union.

The seeds are treated with a solution of mineral and organic fertilizer which solidifies and forms a porous 'envelope' around each grain. Protected by this covering, which allows air and water to penetrate, the seeds can be sown in autumn, instead of in spring, and will resist extremely low winter temperatures. During the first warm days, the seeds start to germinate and grow very quickly.

Experiments carried out by the Soviet scientists resulted in a 30 to 50 per cent increase in crops of cabbages, onions and other vegetables grown from seeds treated in this way.

UNESCO

THE PEASANT AND THE PAST*

By Prof. E. ESTYN EVANS *Dept. of Geography, The Queen's University, Belfast*

I have chosen to speak on a subject which is not without its bearing on the Association's Food and Population Symposium, for though my title makes me look back into the past, peasant attitudes must be reckoned with in any full discussion of the problems raised. Moreover, in the scientific view, all stages and aspects of man's use of his environment are relevant to world problems of food and population. A growing curiosity about the whole past of man on earth is a feature of our age, stimulated by the measurement and exploration of prehistoric time which go hand in hand with the more spectacular exploration of outer space. The world society which is painfully emerging calls for a world perspective of human history, and for archaeologists and historians who can survey the past from a universal viewpoint. As Grahame Clark has recently suggested in an appreciation of O. G. S. Crawford's contributions to archaeology, it was surely his global training as a geographer that made him a life-long advocate of world prehistory.[1] And it is a filial duty as well as a pleasure to recall, at this Cardiff meeting of the British Association, how deep a debt prehistoric studies in the widest sense, in Wales and far beyond, owe to the teaching and example of H. J. Fleure, first holder of the Chair of Geography and Anthropology at Aberystwyth. His interest in peasant communities and in folklife studies, for which Cardiff—thanks to St. Fagans and the missionary zeal of Dr. Iorwerth Peate—has won high renown, is but one facet of his broad vision of human society. I would refer in particular to an essay published nearly a quarter of a century ago[2] in which attention was drawn to many of the universals of peasant life, a theme which Redfield and others have since expanded. Moreover, Aberystwyth and other schools which inherit the Fleure tradition have made significant contributions to social anthropology through surveys of our own societies, the most recent example being the four studies contained in *Welsh Rural Communities*, published by the University of Wales Press.[3]

Looking at the world today, we cannot fail to be impressed by the expansion of urbanism on a scale previously unknown, and by the continued withdrawal of population, in many countries, from rural areas. We would do well therefore to remember that some two-thirds of the world's population are still primary food-producers and that most of them fall within, or are only recently emergent from, the form of society to which the term peasantry is applied. Admittedly it is difficult to define a peasantry. We readily apply the word to the cultivators of many parts of Eurasia but hesitate to use it for any of our own rural people. It is not that peasant folk may not be found in some of the corners of our islands but rather that the word has by custom been applied, and was originally confined, to the lowly cultivators of foreign countries. It carries a connotation of rustic inferiority and has almost become a term of international abuse. There is a story of a missing Soviet delegate to a United Nations conference in New York who, when found, exhausted and dejected, confessed that he had been searching Long Island for the American peasant. At the other extreme the uncritical idealisation of peasant life, typically by writers far removed from the soil, would be unworthy of notice were it not that a mystic peasant-worship was part of the Nazi creed of blood and soil, propagated and exploited for political and military purposes.

The American peasant may be hard to find, but it is ironic that while European anthropologists have ransacked the Old World in their search for 'primitive' societies, it is their American colleagues

* Address delivered to Section H (Anthropology) on September 1, 1960, at the Cardiff Meeting of the British Association.

who have been more concerned with peasant communities, in Europe as well as in Spanish America. One of their purposes has been to analyse the American heritage and the process of acculturation in the light of the European donor cultures. In this connexion George Foster stresses the significance of the time-sequence in determining the selection of imported traits, and the advantage accruing to those cultural items which are carried by the first waves of immigrants.[4] The subsequent 'hardening' of colonial cultures is a process which can be illustrated in Ireland, for example, both from historic and prehistoric times. Folk tradition assists in the identification of the sites first occupied by the bearers of novel culture-traits by endowing them with a sanctity such as was attached, for example, to Tara and Brugh na Boinne.

The value of folklife studies for the interpretation of the prehistoric past has been stated, perhaps overstated, by A. W. Brøgger, who wrote: 'to be able to approach any understanding of the culture of antiquity it is absolutely necessary to begin with what we know today—to know the elements of the peasant culture. Without knowing something of this, one will never attain to the heart of anything confined within the ancient culture.'[5] Twenty years ago, when Brøgger wrote, this approach was something of a novelty, but today it stands vindicated in the co-operation of ethnographers in the brilliant interpretation of Scandinavian excavations, as in the admirably full reports on a migration period settlement on Gotland.[6] As the techniques of excavation become more refined, archaeology is increasingly confronted with evidences of everyday life and ritual for whose interpretation surviving or remembered folkways often provide the clues. We may remind ourselves that less than half a century ago it was felt (writes Collingwood) that unwritten sources of history would give results 'only on a very small scale . . . and only about low matters like industry and commerce, into which an historian with the instincts of a gentleman would not enquire'.[7] Exciting new methods of dating the past have been developed in recent years. Less than thirty years ago at York Randall-MacIver, asking from this chair: 'What means have we of establishing a chronological scheme before 3500 or possibly 4000 B.C.?', could 'answer unhesitatingly that unless earlier written records or traditions come to light it is probable that we never shall have any'[8]

In his presidential address to this Section last year, again at York, Professor Ian Richmond, while excluding 'the majestic trio of ethnology, anthropology and folk-lore' from the true subjects of archaeology, went on to speak of the need to bring them all closer together and of the value to archaeology of analogies drawn from the cultures and habits of 'the wilder peoples of the earth'.[9] I must confess that in my enquiries I have had to be content for the most part with the 'wild or mere Irish'. It is not only because I wish to avoid the dangers of drawing fanciful and far-fetched analogies from distant communities that I shall restrict myself mainly to a region with which I am familiar, but also because, believing adjustment to environment to be a prime factor in cultural differentiation, I want to consider the continuity and adaptation of folklife in a particular environment. Some social anthropologists, who concentrate their attention on such matters as kinship patterns to the exclusion of ecological relationships, seem to blind themselves to the realities of time and space. The student of peasant societies and their evolution must look to archaeology to supply the time-dimension where history is lacking, but equally he cannot afford to neglect the regional and seasonal rhythms of climate and crops, of husbandry, land-use and leisure. There is of course the danger that regionalism may degenerate into parochialism and a narrow nationalism: in groping backwards through our own past we may easily fall into the trap of regarding civilisation as the product of local evolution. We should therefore not ignore the warning light of the diffusionists, however provocatively it is brandished.

It is not that peasants can never be innovators. Margaret Hodgen's well-documented study of the rôle of diffusion in the economic history of England led her to the conclusion that the communities which initiated the Industrial Revolution 'were possessed of a common stock of technological skill and proficiency, transmitted to them by social inheritance from a line of innovating forbears, stretching back in some cases to the twelfth century, in others into the mists of the prehistoric past'.[10] Miss Hodgen concludes that the mediaeval innovators were the peasants and part-time craftsmen of areas of marginal agriculture, and she demonstrates a repeating pattern of distribution along a belt from Yorkshire to Devon where acceptance of

change was stimulated by cultural cross-fertilisation as well as by immigrant peoples and ideas. To Sir Cyril Fox's famous zones we should perhaps add a Zone of Hybridisation which would embrace both the Wessex culture and the dark satanic mills of the Pennine valleys.

The interpretation of archaeological data in its sociological setting is, as Gordon Childe insisted, a major task of research, calling for co-operative effort. Different levels of economy and technology such as archaeology can reveal have been equated, notably by Julian Steward,[11] with ecological conditions, population densities and socio-cultural types; and such basic regularities, once securely established, can be a guide to culture-history. Social forms, however, tend to persist despite changing economic conditions, and such generalisations, if they are more than platitudes, will usually be found to break down at some point. Robert Redfield's concept of a peasantry as a part-society standing in a two-way relationship with an élite is useful both to anthropologists and to archaeologists, but it should not be forgotten that peasant communities show strikingly different systems of social organisation. Not only do techniques of exploitation and systems of land tenure differ widely but the urban element may be strong or weak, or the élite may include pastoral overlords and trade be conducted in periodic fairs. Peasants living under the absolutist régimes which Wittfogel[13] links with the hydraulic civilisations of the Old World dry belt stand in a very different relationship with the élite from those who, under a clan system, claim kinship with their chieftains by virtue of the fact or fiction of a common ancestor.

Although there are apparently a few present-day peasantries who do not use the plough, these exceptions can be explained by difficulties of terrain rather than ignorance or incapacity, and it is perhaps fair to assume that where in the prehistoric past we find evidence of the plough, and of traction animals, we can think of some kind of peasantry. Another useful archaeological index is provided by the hoe. Some types of bronze implements, it may be observed, could have been used as hoes, and it is worth recalling that the very word *palstave*, as John Evans relates,[14] was adopted by Scandinavian archaeologists from the name given to a narrow Icelandic hoe or spade. If the hoe is unaccompanied by plough or spade we may suspect that we are not dealing with a true peasantry; but the plough did not drive out the hoe.

The hoe is a 'gathering' tool appropriate to women's functions. In many parts of the tropics today, where cultivation is left largely or entirely to the women, the short-handled hoe is used almost exclusively. An efficient digging spade, on the other hand, as distinct from a hoe, digging-stick or paring tool, requires the use of the foot and is characteristically a man's implement, calling for heavy footwear. Or a wooden step or peg attached to the lower part of the shaft may be provided to take the thrust of the bare or thinly-shod foot; for instance the narrow one-eared spade used for field work in the south-west of Ireland has a wooden step (Fig. 1:1). But whereas the symmetrical two-eared digging spade—in Ireland* as in Britain—is normally worked with the left foot, the Irish spade or loy has its single wooden foot-step on the right side. This narrow spade is primarily a hand implement, held on the left side of the body, and in this respect as in function—turning and breaking sods and making ridges—it resembles the hoe. The hoe or mattock as well as the narrow spade are traditional agricultural implements in many parts of Highland Britain and Ireland, where women long continued to take a share in field work and where many ancient peasant ways lingered on. Even the narrow spade, however, is not now regarded as a woman's tool, and it is an interesting reflection of man's increasing rôle in the fields, and in the heavy work of making potato ridges, that while women were normally barefooted well into last century—though they wore soleless stockings in winter—the men usually wore brogues.[15] I shall take up the Irish spade again later on.

As to when the emergence of a peasantry in western Europe should be dated, opinions differ widely. Some historians, ignoring as they generally do the evidence of archaeology and oral tradition, have contended that even in the fifteenth century A.D. Irish society was too pastoral and nomadic to permit a true peasant population to evolve. On the other hand the philologist, in identifying a stratified society of overlords and villagers in Neolithic Europe, is dragging the prehistorian by the tail.

* The Irish two-eared spade invariably has iron footplates, which are relict features. (See Fig. 1 : 3, 4.) The shaft is long and either 'unheaded' or crutch-handled: the closed handle is not found.

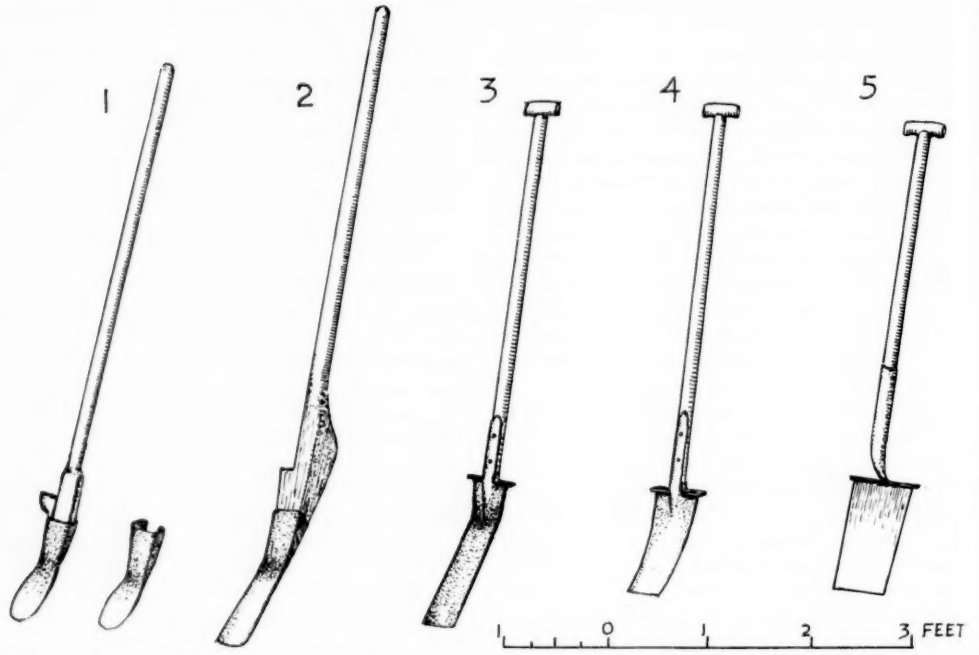

Fig. 1.—*Irish spades.* 1. Co. Kerry. 2. Co. Longford. 3. Co. Fermanagh. 4. Co. Down. 5. Spade made in England for Irish market.

L. R. Palmer, for example, postulates the establishment of Indo-European institutions, with both individual and collective land-holders, in the Danube basin in the third millennium.[16] Archaeology, indeed, reveals that clusters of from ten to fifty houses were the usual form of settlement in Neolithic times in Europe north of the Alps, but single-farms and (presumably) individual holdings do not appear—in England at least—until late Bronze/early Iron Age times. I have suggested elsewhere that these house-clusters may have persisted as the 'clachans' of Atlantic Europe, occupied by kin-groups who for various reasons were more independent than the neighbourhood-groups of the larger villages which developed in the richer lowlands of Europe north of the Alps.[17] If the prehistoric overlords were warrior-herdsmen they may have long scorned permanent settlement. There are hints in the Irish Annals that a roving life was a requirement of the chiefs and cattle-raiding a ceremonial obligation. Excavations have shown that some of the open-air cooking-places or *fulachta fiadha*—the deer-roasts of folk tradition—can be ascribed with some confidence to the middle Bronze Age.[18] Taking into account, further, the extremely archaic structure of early Irish society,[19] and the high proportion of non-Indo-European syntactical and phonological features in the Gaelic language, it seems not unlikely that early Indo-European institutions and some form of Celtic tongue had obtained a foothold in Ireland, as Hawkes and other archaeologists have suggested, by the middle of the second millennium.

By the late Bronze Age, when a marked regional variety in metal technology points to agricultural settlements that were becoming more permanent, we may think of hoe cultivation as supplemented by a light plough whose very inefficiency—together with an abundant supply of animal manure—would have encouraged that continuous 'infield' cultivation which has been the mark of traditional agriculture along the Atlantic fringe of Europe. Another

Fig. 1A.—Key to Fig. 1. (Boundaries based on K. Danaher.)

period of innovation and agricultural expansion coincided with the coming of Christianity, when the heavy plough makes its appearance in the archaeological record. There is evidence for widespread clearance of lowland forests during the second half of the first millennium, the great age of the raths. The older layers of the population had now been absorbed in the Gaelic system. It is significant that by the time Christianity was adopted the *tuath*, which had previously been a social term, had come to have a territorial meaning; and by the seventh century we may think of a peasant economy and peasant values as established over much of the Irish lowlands.

The general acceptance of a universal religion by a rural population is itself, perhaps, an index of a critical level of cultural and economic development, of peasant relationships with an élite. Christian scribes were soon committing to writing the myths, traditions and legendary histories of the Gael. Old rites and ceremonies were Christianised and the vitality of a great body of folklore was renewed by metamorphosis, as is illustrated by the holy wells, by the super-human deeds attributed to the Irish saints, and by the rush charms significantly known as St. Brigid's Crosses. This saint has special associations with cows, a point of interest because, among many groups of lowly food-producers who cannot be called peasants, women are not permitted to milk animals. It seems likely that men's interests were deflected to cope with a more complex technology, with the heavy plough and the oxen that were needed to pull it, and that cows now passed to the special care of the women. (The stealing of cows, however, traditionally an aristocratic sport, continued to engage the energies of young men until the upper strata of the old Gaelic society were virtually destroyed in the early seventeenth century.)

The farmyard hen which was probably introduced some time during the early Christian period was another important addition to women's cares, and it is typically despised by men. In one interesting group of folktales hens figure as fickle strangers, for it is told that every night when they make complaining noises before settling down they are discussing plans for flying back home. Each night some excuse of weather or unpreparedness is found and the hens decide to stay in Ireland for one more day. Now the home they never leave for is always Norway. It is indeed possible that the Vikings carried hens with them—as other sea-voyagers have done—but this is perhaps one of the many instances of transference where 'the Danes', whose name is attached to numerous folk-beliefs and field-monuments, stole the thunder of earlier innovators.

Fertility rites of megalithic antiquity, too firmly rooted to be destroyed, were in some cases won over by the church. There are still to be seen, in a number of old churchyards, sub-megalithic slab-enclosures known as Saints' Beds, a name which recalls the term Dermot and Grania's Bed applied to many megaliths, in which the eloping couple are said to have slept on successive nights. The Saints' Beds are reputed to have had the power of curing barren women who spent the night in them. That the megaliths themselves also preserved this gift is evident from folk beliefs: it was recorded early last century, for example, that 'if a woman proves barren, a visit with her husband to Dermot and Grania's Bed certainly cures her'.[20] It was believed that old burial places were haunted by the spirits of the dead—sometimes identified as fairies—who were waiting to be reborn. Such beliefs may assist

the archaeologist in his task of interpreting his discoveries; and here it is fitting to refer to the brilliant work of Sir Cyril Fox in recovering from excavations in South Wales significant clues as to human thought and behaviour in prehistoric times.

In the peasant world I am speaking of every important task had its attendant ritual which was regarded as necessary to ensure success; and the fulfilment of these tasks was stimulated by the associated rites, as for example the cutting of the last sheaf and the spirit of intense competition which it invoked. The toil of seedtime and harvest, and of the processes of preparing food, was worship as well as work; and delight in a well-finished task is the theme of many a folk-tale. The element of magic, it seems, sometimes outlived the task it accompanied, or was transferred to a similar process: the well-known divination magic of the lime-kiln, for instance, was presumably taken over from the corn-drying kiln when this fell into disuse. Thus many beliefs and customs, at first sight meaningless, may provide clues to practical functions that are now lost. The original purpose of the stone-basins or bullauns found in all parts of Ireland, many on early Christian sites, has long been forgotten, but they are believed to have curative powers and to bring good luck if stones lying in them are turned in the right direction, sunwise. This action seems to support the theory that they were mortars used in the preparation of food. The straw dress traditionally associated with special occasions such as marriages and wakes is probably a relic of peasant dress similar to that still occasionally worn in some districts of Galicia, a part of Atlantic Europe intimately linked with Ireland in early times. Again, certain wild sources of food such as nettles, now taken for medicinal if not magical reasons, were, as A. T. Lucas has shown,[21] a recognised food in early Christian times, and they were resorted to in times of famine down to a century ago. And the cutting and pounding of gorse as winter food for livestock—now regarded as having medicinal value—may be considered the last relic of leaf-harvesting for fodder,[22] which is in all probability a very ancient custom in many parts of north-western Europe.

In a more general way, where a vague sense of 'good luck' is attached to a current practice there is a presumption that we are dealing with an old custom. The Irish addiction to spring ploughing is no doubt partly explained by the climate and the nature of the crops, but I think it may also be related to the practice of burning the land—necessarily in the long dry springs—which was very general down to last century and which perpetuated, I believe, the 'slash and burn' system of migratory agriculture. In this context we should notice that a number of flowers and shrubs which are powerfully endowed with luck are long-established weeds of cultivation historically and emotionally linked with man's farming activities. They are also traditional indicators of the safe and proper times for beginning specific tasks. For ecological reasons, the flowering of familiar plants and the movements of migratory birds are critical times in the countryman's calendar, guiding the beginning and ending of seedtime. The Christian calendar has partly replaced that of nature—for it is St. Patrick's Day that marks the planting of the first potatoes—and the clock has ousted the cock from the kitchen, but in the traditional peasant world time is not yet measured solely in terms of man-made units, whether of weeks or hours, or of money. It is phenological, latitudinal and latitudinarian, more akin to the poet's time than to that of the mathematician or the economist:

> 'Tis time, I think, by Wenlock town
> The golden broom should blow;
> The hawthorn sprinkled up and down
> Should charge the land with snow.

For gold and snow, read butter and milk. The lone may-tree or fairy thorn is deeply venerated in many parts of Ireland, and the explanation lies, I believe, not only in its promise of warm weather—the flowering hawthorn is a pretty sure sign that damaging frosts are over—but also in the magic beauty of its bounteous creamy blossoms, symbols of a rich flow of milk. The primrose and marsh marigold (and the buttercup itself) have brightened springtime with the promise of golden butter, and the dandelion—the flower of the milkmaid Brigid—was twice blessed for its yellow flower and its white milk. Similarly, for the peasant of the Western World, summer took its leave in a blaze of symbolic colours, in the red berries of thorn and rowan. Many such beliefs and related customs were focused on the beginning of the year's ecological halves, in early May and early November, which were the times when sowing and harvest were celebrated, transhumant stock assembled and rents paid in cattle. The present economy respects the past, for

in Ireland, in town and country alike, the gale-days when we are expected to pay our rents fall on May 1st and November 1st. More widely, these rural festivals have been adopted and adapted by urban cultures in such varied celebrations as May-day and Guy Fawkes' Day. The sophisticated culture of cities renews itself from time to time, also, by drawing on folkways for inspiration in art, sculpture, music, literature and drama. Conversely, as has been demonstrated from many parts of the world, the material culture of the élite—most obviously the easily copied modes of dress—filters slowly into the rural communities.

If we are to see peasant life in its environmental relations and its archaeological relevance the collection of calendar customs, the analysis of questionnaires, or the typological study of material culture, however valuable, is not enough. To interpret these tools and customs we should see them in relation to the whole culture as it has evolved in a given environment. To do this we need to live as closely as possible with the folk-culture we are studying, to share the tasks and handle the tools which are part of it. Not to lend a hand when some critical operation such as butter-making is in progress—to take one's turn at the churn-staff or at least to lay one's hand on it as a token of good-will—is to remain a stranger, under suspicion of bringing ill-luck. I should like to give an example of what has been called, somewhat derisively, 'the shirt-sleeve approach' to anthropology. (Shirt-sleeve archaeology, on the other hand, is eminently respectable.) I have a vivid recollection of assisting one stormy day in 'shearing' a tiny field of oats in Donegal. I noticed the reverent attitude towards the weather ('"Tis windy, thank God') and I recalled that, had it been a few generations earlier, I might have shared in the reaping blessing recorded for the Hebrides: 'the father of the family, taking up his sickle and, facing the sun, cutting the first handful of corn, and, turning it three times round his head, sunwise, the reaping blessing was sung by the whole family, praising the God of the harvest for corn and bread, food and flocks, wool and clothing, health and strength, peace and plenty.'[23] Using the toothed shearing-hook—the sickle—one holds the standing corn in the hand when cutting and pulls the sickle smoothly towards the body so that no grain is lost by shedding. The straw is cut clean and low, for oatstraw has many uses and in a subsistence economy is well worth the time taken by this slow method of reaping. Straw tying-bands which had been made ready were lying on the stubble behind us, all pointing in one direction, across the wind. Three handfuls of cut corn were laid on each band and bound into sheaves which were carefully placed in the prescribed direction, their butts to the wind. This practical lesson on the virtues of the wind was followed by the remark that the best harvest weather that could be hoped for was 'a scourging wind that would blow the stooks to the haggard'.

Much can be learnt by handling tools because their functions are not always obvious from their shapes, and I have met Englishmen who have refused to call the narrow Irish spade a spade. If a museum archaeologist with no field experience were given the task of arranging for display a collection of worn unhafted spades—'precise circumstances of finding unknown'—he would probably place them in chronological order from 'Early Iron Age' to recent. In fact a series such as is illustrated in Fig. 1 represents a cross-section of the country from Co. Kerry to Co. Down. South-west of a line from Co. Galway to Co. Wexford the one-eared spade is generally used, while the two-eared spade becomes common north of this line and is used exclusively north of a line from Co. Sligo to Co. Louth.[24] One would not suppose from the great variety of spade-types which exists—a single foundry was recently turning out 200 different shapes—that nearly all types have essentially the same function, the construction of cultivation ridges or lazy-beds. Our museum archaeologist would probably not be far from the truth in his hypothetical evolutionary sequence, but without a knowledge of the contemporary scene he could go wildly astray with his dates. It is our duty to record such evidence in the field before it vanishes.

The long 'unheaded' shaft and the wooden step of the one-eared spade are archaic features whose retention can be partly explained by remoteness and rural conservatism, while the minor variations in both groups of spades can often be directly related to local soil conditions. Thus a tapering blade is adapted to light stony soils whereas an expanded mouth suits heavy clays and wet peats in which a tapering blade would stick like a wedge. It should be added that these narrow spades are well designed for undercutting tough sods and for loosening the deeper layers which, in heavily

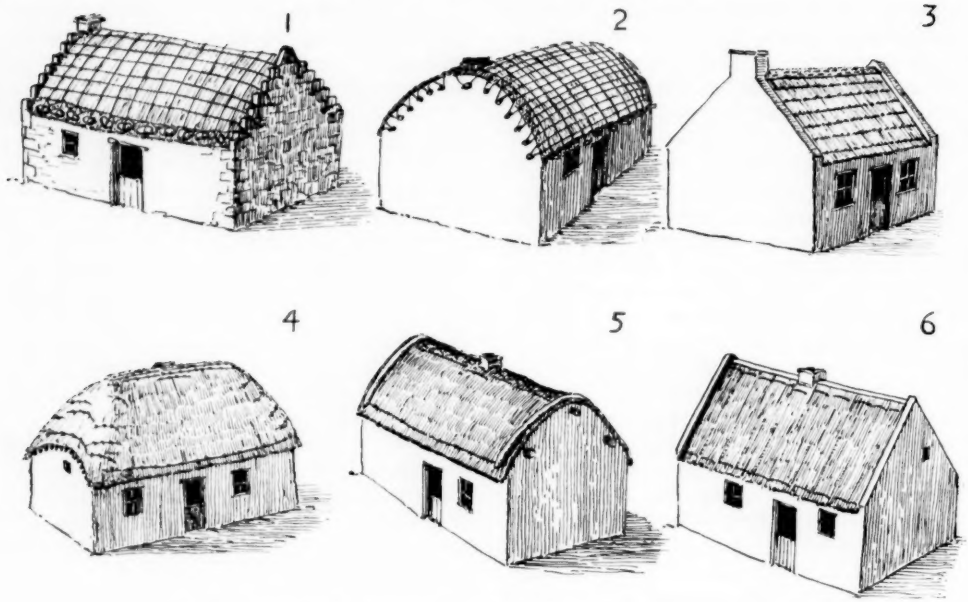

Fig. 2.—*Thatched roofs, Ireland.* Nos. 1–3, gable chimney; 4–6, central chimney.
1. Mayo (roped, stone-weights). 2. Donegal (roped, stone pegs). 3. Antrim (pegged fir-ropes, sod-ridging). 4. Cavan (hip-roof, scolloped). 5. Armagh (purlin-roof, scolloped with mud-coping). 6. Down (stappled mud-thatch).

Fig. 2A.—Key to Fig. 2. (Boundaries based on K. Danaher.)

leached soils, tend to become panned. They are supplemented by the long-handled shovel for lifting and moving soil, and these tools are the traditional fashioners both of the lazy-beds and of the earth-and-sod banks which are landscape features of considerable interest to the archaeologist. Such banks, strengthened by temporary fences for which they provided a footing, seem to be an ancient and persistent form of field boundary. It is interesting to notice that the thorn hedges of the present-day fields are almost invariably planted in hedge banks, and that Irish hedgerow trees are therefore, as Arthur Young observed, characteristically stunted in growth as compared with the hedgerow trees of England.

Before leaving the Irish spade I would relate its specialised sod-paring function to an almost universal feature of the traditional thatched roof (Fig. 2), its undercoat of thin tough sods which, cut in long strips, are laid grass-upwards on the timber roof-frame. In the south the heavy hoe and in the north the flachter or breast-plough were also used for the special purpose of paring roof-sods. Such long rolls would go well with a curved roof supported directly on crucks, and the whole structure would be portable, for the turves can be rolled

and carried on a stout stick like a carpet. The cruck form of the Lapp turf hut immediately comes to mind.[25] Even as late as 1700, in a part of Ulster where groups still practised a form of transhumance, we read that their cabins were 'built so conveniently of hurdles and long turf that they can remove them in summer towards the mountains, and bring them down to the valleys in winter'.[26] The thatch of various styles which now accompanies the sod undercoat should perhaps be regarded as an addition to the house marking the attainment of fixed settlement and of peasant status and skills. It was knowledge of the Irish peasant roof that led Dr. Bersu to suggest an explanation for an inverted sequence of artifacts encountered during his excavation of a rath-house at Lissue in Co. Antrim. Worked flints of Neolithic type found overlying a 10th century habitation layer could well have come from the sods of the collapsed roof. The same excavation provided another example of the value of folklife study for archaeological interpretation: the purpose of a stave-built vessel recovered from a still earlier level, tentatively identified as a butter-churn but fitted with attachments for suspension, was clarified by evidence that, in a neighbouring district, butter was made down to a century ago by swinging the churn from the rafters.[27]

Another example of local variation within what is basically a single culture area is provided by the traditional peasant house-types of the region best known to me, the north of Ireland. The intimate adjustment of elements of material culture to environment is one of the characteristics of peasant society which have given it continuity and means of survival. The archaeologist should be aware of such regional variety and take note of it. The ground-plan of the peasant house shows little variation from place to place, but the materials of the walls, the slope and construction of the roof and the methods of applying and securing the thatch display marked regional adaptation in which factors such as exposure, and the availability of stone, timber, mud and various thatching materials have played their part (Fig. 2).

Finally, in considering the continuities of peasant life we cannot fail to notice the remarkable powers of memory and the colourful speech of the Irish countryman, reflecting the strength of an oral tradition in which phrases were designed to be memorised. Where the accumulated experience of the past is stored in the memory as a guide to proper action knowledge becomes a function of age and bestows prestige and authority on the old. Of the attributes of Irish peasant society which social anthropologists have described[28]—deference to the aged, the simple festivals and entertainments, the solemn formulae and blessings that go with daily encounters and journeys, the match-making, the high birth-rate, the reverence, modesty and prudence, the emotional stimulus of communal effort, the attachment to family and kin and to ways of sharing the resources of nature—there is little doubt that a good number have persisted from prehistory. The archaeologist, then, along with others, can profit from the efforts that are being made to place this peasant world on record before it succumbs to the accelerated forces of change.

In the organisation of folklife studies in these islands Ireland has won renown thanks to the pioneer work of the Irish Folklore Commission, carried out for the most part in the Republic. But lest it be thought that the more commercial and industrial community of the north has no concern for its past I would refer to the enlightened action of the Northern Ireland parliament in passing a Folk Museum Act (1958) which makes provision for establishing a museum and maintaining it by funds contributed both by the Ministry of Finance and by local authorities. I believe this Act is unique not only in its purpose but also in having had the enthusiastic support of all political parties in the Northern Ireland parliament. It is the aim of the Trustees to establish a museum which will be not only an attractive display but also an archive and a centre of research, for the illumination of the past, the education of the present and the inspiration of the future.

Peasant attitudes must certainly be reckoned with in attempts to improve agricultural production and standards of living in many parts of the world. There is a danger that in trying to alleviate poverty and undernourishment spiritual values and social cohesion may be destroyed. Agrarian reform needs to be something more than an economic process in communities where farming is primarily a way of life and not a business investment. And where population is very dense, it seems that mechanisation of the kind which reduces output per acre while increasing output per man offers no solution, even to the problem of raising material living standards.

Under these conditions increased productivity can perhaps best be obtained by improving agricultural practice within the existing institutional framework. It has been argued for example for Ceylon that, under present conditions, to forbid peasant fragmentation of holdings—at first sight a root cause of poverty—would cause not only social disruption but increased economic distress.[29] Comprehensive regional studies of peasant societies, therefore, are needed as a preparation for the future no less than as a guide to the past.

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MONETARY THOUGHT AND MONETARY POLICY IN ENGLAND*

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The purpose of this paper is to review certain basic notions that have underlain English monetary policy during the nineteenth and twentieth centuries. I shall contend that the theoretical approach to the problems of policy needs redirecting in the light of changes in the institutional and political environment. I have nothing revolutionary to say: the redirection which I suggest implies no condemnation of the good sense of our forefathers. I shall not call in question their insistence on the connection between the internal and external objectives of monetary policy, but shall confine myself to the considerations relevant to a closed system.

Throughout the period—we can date it from the Bullion Report of 1810—the broad approach of every generation has been a Quantity Theory approach. I take this in the most general sense of a belief that the value of money is dependent, to an important extent, on its quantity, and that therefore some control of the supply of money is an essential pre-requisite of any sensible monetary policy. The Quantity Theory, in all its simplicity, has great attraction. It is a supply-and-demand analysis, with a demand schedule whose peculiarity derives directly from the concept of money not as an object of exchange but as the medium of exchange. That this is only a small part of the truth is acknowledged even by those economists who rate it most highly, and I should be guilty of travesty if I were to accuse authoritative writers of allowing the Quantity Theory to monopolise their thoughts on monetary action. But I am going to suggest that we should all have a clearer view if we went back to the starting-point and looked afresh at the basic concepts. We must go back to the nature of money and demand for it.

The word 'money' is used in two senses. Money is the standard by which we measure the exchange values of things and in which debts are denominated. This is the abstract sense in which we use the term; we know what pounds, shillings and pence are, in the sense that we know they are the units in which debts and prices are stated. We run into difficulties—the familiar index-number problem, on which I shall not comment—when we begin to generalise about 'the value of money', still using the word 'money' in this abstract sense, but at least there is no doubt about what is the 'money'. When we say that our economic system has acquired its present shape on the basis of a general assumption that the value of money is reasonably stable, it is of this abstract sense that we are thinking. Yet when we say that, in order to achieve this stability in the value of money, we must control the supply of money, we have switched away from the abstract sense of money to its other, concrete sense. For by 'control of the supply of money' we mean control of the availability of certain assets which are used as media of exchange and stores of value. We have switched from an unambiguous abstraction to a class of marketable objects whose boundary has neither sharpness nor certainty nor permanence. And we have arrived at one of the perennial questions of controversy: the question of what assets are, and what are not, included in the 'money' category. I suspect that, because money in the abstract sense is an unambiguous concept, people have subconsciously believed that there must also be a simple answer to the question of what is money in the concrete sense. They have been looking for a sharp line of distinction where only Marshallian shading has reality. This pursuit of a will-o'-the-wisp has had its effects on discussions

* Presidential Address delivered to Section F (Economics) on September 2, 1960, at the Cardiff Meeting of the British Association.

of policy, for people have been too ready to believe that there is an identifiable quantity the control of which is all-important.

The difficulty of identification has derived from the twofold nature of money (we are now speaking only of the concrete sense) as a medium of exchange and as a store of value. Money is both these. An asset which entirely ceases to be a store of value ceases to be used as a medium of exchange, as has been seen when a money is rejected in the final stage of hyper-inflation. There are articles which are stores of value but are clearly not money because they are *never* used as media of exchange. But are we to label as money all other stores of value, that is to say all those which are ever brought into use, whether commonly or occasionally or only rarely, as media of exchange? The usual answer is that we should include as money only those assets which are commonly used as media of exchange. Resort to the adverb 'commonly' at once emphasises the absence of any sharp line of distinction. Are balances in a clearing bank to be labelled money while deposits in the Post Office Savings Bank are not? I find it impossible, on the evidence of recent practice, to find any watertight reasons for so distinguishing. It is sometimes supposed that the distinction can be made to turn on the simplicity of the procedure for transferring the asset. When I take a coin or a note out of my pocket, and hand it across the counter, I am using money to make a payment. When I draw a cheque on a current account at a bank, economists would certainly say that I am using money to make a payment. But if this act of writing an instruction to a bank is a use of money, why not also written instruction to the Post Office Savings Bank or a Building Society? And what could be more simple than the showing of a consumer's credit card? Nor is it altogether satisfactory to retreat to the fact that many Post Office and Building Society balances lie for long periods undisturbed, for this is also true of many balances in the Clearing Banks. Especially this is liable to be true of deposit account balances, yet most economists would regard them as money, equally with current account balances, and the procedure for paying them over to other people is scarcely less simple than drawing a cheque on a current account.

Nor is there conviction in the notion that the line can be drawn by reference to whether interest is earned. Present English practice is that interest is not paid on current accounts, but this has not always been so, nor does it universally apply in other countries where banking competition is more free. Even in present English conditions, large sums are held in current accounts as minimum balances, ordinarily undisturbed, as inducement to the banks to operate accounts without charge: interest is thus virtually paid and offset by turnover charges. Whether the question of what is money is made to turn upon what has been common usage among economists, or on whether an asset is commonly used as a medium of exchange, or on whether it yields interest, it is not possible to give an unambiguous answer.

A similar attempt to escape from the difficulties of identification of money is to be found in the distinction sometimes drawn between 'active money' and 'idle money'. The actual division of bank deposits into demand deposits and time deposits is often thought of as a statistically convenient approximation to an economically significant division into active money and idle money. But this distinction is at best misleading. No asset is in action as a medium of exchange except in the very moment of being transferred from one ownership to another, in settlement of some transaction, and no class of asset used in this way can logically be excluded from the class of active money. Between transactions, all money is idle. Yet if activity is held to cover the state of being held in readiness against possible use in exchange, then all monetary assets are active all the time. It is not merely that we cannot easily earmark for statistical assessment the quantity that is active: there is no such quantity, except in the all-embracing sense of all those goods or claims regarded by their owners as potentially useful for settling market commitments. It is this wide concept of liquid assets that we must put, in the place conventionally occupied by 'the supply of money', as the monetary quantity influencing total effective demand for goods and services. And we must interpret it widely enough to include credit that can be brought into existence concurrently with a decision to exercise demand.

When all the assets included in this class are reviewed in detail, we find that they include not only the deposit liabilities of banks but also the readily-callable liabilities of a large and widening

range of other financial intermediaries. The ease with which these intermediaries enlarge their balance-sheet totals is as relevant to the monetary situation as is the behaviour of those financial intermediaries we call 'banks'. Nor should we limit the range to firms ordinarily regarded as financial intermediaries. Among firms whose main activity is manufacturing or trading, most do a great deal of lending and borrowing: they extend 'trade credit' to their customers, and they take 'trade credit' from their suppliers. Many are active on both sides of the account, and nearly all are involved in credit transactions on one side or the other. The distinction between banks as creators of credit and other firms as users or intermediaries in the monetary field is if not completely false at least misleading. From the point of view of the pressure of effective demand, the crucial step is that which increases the power to acquire goods and services, on the part of people inclined to exercise it immediately; and banks are by no means the only firms to place this power in the hands of others.

That trade credit, in the sense of credit granted by firms whose primary business is non-financial, is an effective element in the supply of purchasing power, is a view sometimes conceded but with the odd qualification that it is only 'net' credit that counts in this way. If, for example, a statistical measure is sought, it is supposed that only the excess of a firm's claims over its debts to others should be counted.* This is a position that cannot be sustained. We are thinking of the power to exercise demand for goods and services, and for this purpose the gross total of trade credit is relevant. An entire closed circle of firms in manufacturing industry may begin giving credit more freely, and all of them proceed to place large orders with each other; there is no doubt about the increase in effective demand, although the increase in credit granted by all the firms together is balanced by the increase in credit taken by all the firms together. No one would seriously suggest that bank credit should be 'netted out' by deducting the debts people owe to the banks, although one of the Tables in the Radcliffe Report curiously errs in this way.†

We should pause here to notice that a distinction between 'credit' and 'capital' is no more helpful in monetary analysis than is the distinction between banks and other creators of credit. This distinction between credit and capital was vigorously argued in the nineteenth century literature of the subject, and there are echoes of it in those contemporary views that draw a distinction between the 'money market' (or 'credit market') where the central bank has its business and, on the other hand, the 'capital market' upon which the influence of the monetary authorities can only be indirect. It is better that we should think of a single market, or at least a single group of markets, in which immediate purchasing power is traded against claims whose variety, in maturity and in other respects, is infinite. In some countries this is a more imperfect, in others a less imperfect market; but the important likeness that links all parts of the market is the trading of more immediately for less immediately available purchasing power. All participants in the market are so trading; all are trading in capital in the sense of purchasing power, and none is trading in capital in the sense of goods. Each part of the market is as fundamentally a money market as any other, and each part of the market is just as much a capital market as any other. All are adding to the supply of credit when the claims against themselves are more immediately useful than the other claims which are the trading counterpart. This supply of credit, which can be immediately used for exercising demand, is no monopoly of the banks; the power of the banks to create credit (and it is credit, not money, that is relevant here) thus provides no justification for control of the banks while other credit agencies are left uncontrolled.

A presumption in favour of discriminatory control of the banks can, however, be established if it can be shown that there is a sufficiently firm proportionality between the volume of bank credit and the volume of other credit. If, when bank lending is curtailed, other lending must necessarily be curtailed in equal proportion, the control of banks

* Cf. J. Viner, *Studies in the Theory of International Trade* (1937). In the course of his review of the writings of the Banking School and the Currency School, Viner says (p. 247) that 'J. S. Mill went too far' in including trade credit in the stock of purchasing power. But Viner concedes the substantial point later in the same paragraph.

† Cmd. 827 (1959), Table 22 (on p. 171). That in twelve months no one should, to my knowledge, have drawn attention to the oddity of column (1) in this Table, is a sad commentary on the extent to which the arguments used in the Report have been understood.

becomes a useful technique, on the administrative principle that their heads are the easiest to hit. If every firm dealing in credit always held absolutely stable the proportionate distributions of its assets and its liabilities—that is to say, if balance sheets always had exactly the same structures—this proportionality of the supply of effective purchasing power to the supply of bank credit could be taken as the basis of policy. In fact, there are two important reasons why this strict proportionality cannot be assumed. The first is that in course of time people devise new financial intermediaries; the efficiency of the financial system increases, and this process tends to accelerate in a prolonged spell of business prosperity. The second—and in the short period relevant for most purposes of policy, this is the more weighty—the relative attraction of non-bank debtors varies according to the general trade prospect. This is of course our old friend 'the inherent instability of credit'; but it is important to recognise that in its modern form it refers not so much to bank credit as to the relation between bank credit and non-bank credit. At a time when, as an anti-inflation measure, the authorities are enforcing a restriction of bank credit, the apparently favourable position of other debtors makes the extension of non-bank credit peculiarly easy, as we saw in England in the middle 1950s.* The important practical question is how easily one source of credit can replace another, and whether such a disturbance sets up any corrective process which will act quickly enough to be useful for purposes of policy.

Before I turn to this fundamental question, I must refer to the use of another concept, deeply embedded in the literature of our subject. This is the velocity of circulation. I have hitherto been formulating monetary phenomena by referring to the supply of and the demand for various assets and liabilities; but many writers have thought it more convenient to refer to variations in demand as variations in the velocity of circulation of money. They think of people not as wanting more or less intensely to hold money but as being less or more quick to pass on money in exchange for goods and services. Put like this, the one concept is simply the reciprocal of the other, and it does not matter which we use. I myself prefer to think in terms of the demand for money balances, because the term 'velocity of circulation' implies closer analogy with a physical process than I can find in the working of the monetary system, and I notice that when economists seek to explain changes in the velocity of circulation they at once begin to talk about the intensity of the demand for money. Like houses and other assets, money is not going round steadily all the time. Instead—again like houses and other assets—it is always resting in the ownership of one person or another, save for the isolated moments when it changes ownership. What people mean when they say that the velocity of circulation has increased is that the volume of transactions has increased relatively to the stock of money (in some sense), or that the national income has increased relatively to the stock of money (the ratio more specifically referred to as the 'income velocity of circulation'). As a label for the purely statistical phenomenon of the ratio of one quantity to another, the term velocity of circulation is harmless enough and so well established in the literature that it would be foolish to refuse the convenience of using it. The trouble begins when we jump to the conclusion that a rise in the ratio of payments to bank deposits means only that firms have decided to put their bank deposits to faster use, whereas much of what has happened is that firms have decided to give more credit and people have shifted some of their savings into the hands of more nimble intermediaries. The artificiality of the concept lies, in short, in its reliance on a distinct and identifiable category of money; and one danger in using it lies in its encouraging us to overlook the relevance, to the pressure of total demand, of sources of credit outside this defined category. A further danger is that the definition normally used being either bank deposits plus notes, which (particularly the notes) come fully into action only late in productive processes, a rise in the velocity of circulation is a

* No comprehensive statistics of trade credit exist, even for joint-stock companies. For companies quoted on the Stock Exchange and engaged in manufacturing, building and distribution, however, some figures can be extracted from the investigations made by the National Institute of Economic and Social Research. For these companies, the balance sheets relating to late 1956 showed trade creditors £m. 2050; as compared with £m. 444 bank loans, and trade debtors £m. 2609 as compared with £m. 649 cash and bank balances. The swings in these figures for trade credit, from one year to another, also dwarf changes in the bank credit and bank balances employed by such companies (*v.* Radcliffe Report, Cmd. 827, paras. 297–311).

phenomenon that lags behind a rise in the disposition to spend. The existence of this lag should warn us against undue reliance on any corrective mechanism set up by 'a rise in the veolocity of circulation'.

The basis of such a corrective mechanism is sometimes represented as the anchorage of the velocity of circulation in the peculiar demand for the medium of exchange. Our examination of the nature of money and of the demand for it has however warned us that there is no single asset or group of assets that uniquely possesses a uniform monetary quality that is totally absent from all other assets. Except for purely didactic purposes it does not help to talk about a normal velocity of circulation any more than it helps to say (which is the same thing) that the elasticity of demand for money is unity. On the other hand, convenience in settling transactions is *an* important quality in a monetary asset; various monetary assets have this quality in varying degrees. If money income rises, or if the supply of some of these assets falls, people will feel some lack of this convenience in relation to the contingent needs against which they hold such assets, and this is the substance to which economists refer when they assume a certain normality in the velocity of circulation. But we must remember that many assets have the monetary quality in varying degrees; if, as the supply of some of them falls relatively to income or turnover, others can easily be increased, the sense of inconvenience is minimised, and we have to ask ourselves whether it remains effective enough to be interesting. We are in fact back at our major question of the relation between supplies of different classes of monetary assets, having merely reformulated, with the help of the concept of the velocity of circulation, the problems we had already traversed.

In our own time most attention has been given to a comparatively late link in the chain of argument: the question of how, as the supply of monetary assets becomes less adequate, increasing inconvenience manifest in rising interest rates provokes a fall in total demand. Although this part of the argument has always had some attention, the almost exclusive concentration upon it is something of a post-1920 novelty. In the great controversies of the nineteenth century the prior question of the relation between varied monetary assets was always well to the fore. In the early part of the nineteenth century one of the major questions related to the connection between the Bank of England's note issue and the issues of the country banks. Thornton, in his *Paper Credit* (1802) had developed the view that the issues by the country banks were substantially governed by the Bank of England's issue.* He may have been wrong in his opinion, but he certainly regarded it as important, because he regarded both kinds of notes as money the supply of which was relevant to the price level. When the Bullion Committee (of which Thornton was a member) in 1809–10 investigated the depreciation of the pound, they adopted this view in one of the more emphatic statements in their Report,† but only two pages later they carefully explained the power of country bank-notes permanently to displace Bank of England notes.‡ They also noted the importance of increasingly effective use of bank deposits in circumscribing the real demand for bank-notes. In the controversy which raged round the Report, and more especially in the controversies of the next three decades, the relation between country notes and Bank of England notes continued to be hotly debated. There were no indisputable statistics that could be used to help settle the argument, just as in our own day there are not yet statistics to enable us to convince each other on the question whether or not the liabilities of the clearing banks are absolutely crucial to the behaviour of the wider structure of credit.

In the next generation the most hotly debated question was the regulation of the issue of bank-notes. In this phase—the Currency School and Banking School controversy—those who argued for strict regulation of the issue of Bank of England notes were no longer prepared to rely upon any automatic dependence of country issues upon Bank of England issues. They argued for—and obtained—strict and direct limitation of the country issues. But this relation of country to London issues of notes now scarcely held the centre of the stage. The main dispute now raged round the position of other claims—bank deposits, bills of exchange, and trade credit generally. The Currency School argued for

* H. Thornton, *An Enquiry into the Nature and Effects of the Paper Credit of Great Britain*, pp. 211–241.

† In the opening of their Section IV.

‡ Cf. Thornton's Chapter IX, where qualifications are as abundant as in any modern survey.

strict regulation of the issue of bank-notes. The Banking School, more diffuse in their remedies for economic instability, were united only in their negative view that the Currency School remedy was invalidated by the fact that bank-notes formed neither the only nor the governing part of the supply of purchasing power. The use of bank deposits, bills of exchange and other forms of credit as substitutes for bank notes had long been acknowledged in the literature; on the historical order of events as we know them, it might rather have been argued that bank-notes were a substitute for bills of exchange. But now that a strong case was being made for strict control of the supply of money, the question which was money and which was not money became of high practical importance; moreover, the spectacular growth of banking was emphasising the weight of bank deposits in the total circulation.

The simplest Currency School view was that only gold and notes were money, and that therefore the circulation of gold and notes together should be made to reflect the changes in the gold supply; this is, as you know, the essence of the 1844 Bank Act. But this is doing the Currency School less than justice. Its protagonists did acknowledge the relevance of banking and other credit, but some took the line that if notes were strictly controlled, deposit banking should be kept in order by some other measures; others argued that the velocity of circulation of banking and other instruments was comparatively so low that these were unimportant parts of the money supply; again others argued that, since gold or notes could be demanded in all final payments, the pressure of purchasing and economic activity could never for long get out of line with the supply of the basic monetary instruments. It was this last view that was destined to dominate many decades; it prevailed against the Banking School view that the components of the money supply were so many and varied, and such effective substitutes for each other, that any attempt to control their quantity was doomed to failure, and would probably aggravate economic crises.

The prevailing doctrine—which gave Britain its basic monetary laws for seventy years—depended on the fact that gold or Bank of England notes could be demanded in all final payments. This derived not merely from the law of 1833: it was deeply rooted in the country's practice for a long time before that. This singular quality of gold and notes became directly important as soon as a general breakdown of credit developed: at other times bank deposits and bills of exchange were generally good enough, but in a crisis a businessman had to be able to put his hands on notes or gold, or he risked bankruptcy. As long, therefore, as a financial crisis was thought a possibility, bankers and other granters of credit paid some regard, in the conduct of their lending business, to the state of the Bank of England's reserve. And just in these decades Bank Rate movements were beginning to be employed by the Bank of England as the method of protecting the Reserve. A rise in Bank Rate became all-important as an index—a warning that a breakdown of credit, followed by a disastrous spell of depressed markets, might be just round the corner.* The Banking School were right in emphasising the variety and the importance of sources of credit, but the Currency School were right in arguing that the superstructure of credit could not for very long get out of line with the supply of the basic money, the gold and bank-notes. In this situation, the broad line of argument developed in the first part of this paper was not important. But the economy was distressingly unstable.

It was also one in which some of the most relevant circumstances were changing. If it was right to assume that the superstructure of credit could not for very long get out of line with the supply of basic money, experience very quickly showed that the possible time-lag could be of devastating importance. Partly because the Bank of England was not using its warning signal promptly enough and partly because the underlying causes of instability were very strong, crises of the first order of magnitude recurred in 1847, 1857 and 1866. This experience led the authorities to modify the Currency School policy to the extent of admitting that the Bank of England must act as a lender of last resort to prevent breakdown of the system. The crisis of 1866 was the last of the old kind, and Bagehot's writings ensured the respectability of

* This interpretation of the *modus operandi* of Bank Rate at this time is convincingly argued by A. B. Cramp in his article, 'Horsley Palmer on Bank Rate' (*Economica*, Nov. 1959), and in his unpublished Ph.D. thesis, (London), 'Opinion on the Operation of Bank Rate, 1822–60'.

the new policy. Nevertheless, Bank Rate continued to retain something of its power as a warning signal internally: this was partly because memories of the old-fashioned crises lingered on, and partly because, although it ceased to portend a breakdown of credit, a high Bank Rate was still apt to portend a spell of trade depression. Not until the effective adoption of Full Employment policies was the warning power of Bank Rate reduced to a ghost of its old self.

The scene was also changing in that the internal effects of Bank Rate were becoming relatively unimportant. In the second half of the nineteenth century London's power as an international financial centre grew fast, and the Bank of England was enabled to perform its duty of keeping the pound on gold by operating principally on the international capital position.* (This had always been an element in the *modus operandi* of Bank Rate, but in the period 1860–1914 it became the principal element.) Some of its direct influence as a signal for business men remained, as I have suggested above; more importantly, the disturbance of the international capital situation disturbed investment in the developing countries overseas, with severe repercussions on the trade of the home country. Just how much monetary conditions really affected the trade cycle it is impossible to say; if we are looking only at the last few decades before 1914 I find myself much in sympathy with Rostow's view of 'an essentially negative element',† for the rise in interest rates often waited until the boom was already cracking, and merely ensured that the depression should be severe. However, without going as far as this we must still admit that the quick power of Bank Rate on the international capital position was the dominant feature, and this change of emphasis was duly reflected in the dwindling literature on monetary questions. Bank Rate as the controller of international gold movements came to occupy a leading place in expositions of the monetary system; the Quantity Theory and the 1844 Act remained alongside the discussion of Bank Rate, but the old and the new were in this phase never really integrated. It was the literature of this phase that was more or less my introduction to the subject, and I well remember my first impression of confusion, whether a rise in Bank Rate increased the quantity of money (which seemed to be the result of attracting gold) or decreased the quantity of money (which seemed to be a necessary part of the deflation a rising Bank Rate was supposed to provoke). My teachers soon put this right, but when I look back at the pre-1920 literature I am not surprised at my early difficulties.

The main development of monetary thought in the course of the nineteen-twenties was the integration of these various elements into a single coherent body of theory. The development originated mainly, I believe, in the pre-1914 academic activity in the study of the trade cycle, though there was also powerful stimulus from the international monetary strains of the 'twenties themselves. Having regard to what we know Marshall to have been thinking at the turn of the century, it is not surprising that the reshaping of monetary thought in the next generation was largely the work of Marshall's successors, especially Lavington, Keynes and Robertson; and we must not forget that English economists were beginning to notice the work of Wicksell and his followers on the Continent.

As a survey of this kind must necessarily concertina many intricate episodes in the history of thought, I will summarise these developments of the 1920s simply by reference to the form in which they emerged, at the end of the decade, in Keynes's *Treatise on Money*. The *Treatise* has between its covers many strands of thought, as those who were students in those days will remember too well; but for our present purpose we may confine our attention to two of them. The first is that the demand for money was refashioned by looking at money primarily as one among other stores of value, the preference for the peculiar convenience money possesses being weighed in the manner traditional in the ordinary theory of value, against the interest that could be obtained on the other stores of value. The second idea we must notice was, along Wicksellian lines, that the rate of interest influenced the balance between the demand for capital and the willingness to lend, thus providing a mechanism whereby the supply of money (and the demand for

* For a contemporary exposition, see e.g. G. Clare, *A Money Market Primer*, 2nd edition, 1905, esp. pp. 103–7 (on effects on international movements) and 36 (for attitude that no disturbance of the internal economy is sought).

† W. W. Rostow, *British Economy of the Nineteenth Century*, p. 57.

it) was linked with the pressure of total demand and so with movements in the general level of prices.

The first of these developments—the liquidity preference theory of interest—represented a clean break from the Quantity Theory, in that money, ceasing to be a pure medium of exchange, ceased to have that freakish unit elasticity of demand, and became something whose value was determined in a way we need not feel strange when we had just been learning a more universal Theory of Value.* But in his formulation Keynes preserved his links with tradition by notionally dividing money into 'active money', whose elasticity of demand was unity, and 'idle money' the utility of which behaved in the more ordinary way and had to be balanced against the interest that could be earned by holding bonds. For the purpose of establishing his main propositions Keynes need not have made this bow to his ancestors, but he did, and I suspect that life would be easier if we forgot about it; I have already noted the artificiality of the distinction. However, its use did facilitate demonstration of an equilibrating mechanism in the system. A rise in prices increased the amount of active money needed, the money available for idleness was thus reduced, the demand for bonds therefore fell, the rate of interest rose, demand for capital goods was checked—and so the rise in prices was stopped. This is the Keynesian system of 1930 in baldest form, and the précis omits many sources of friction continually emphasised by Keynes. In terms of practical policy, the analysis did point advantage in control of the supply of money; to Keynes this meant control of the supply of bank deposits, and his attention to such subjects as the cash ratio in banks and central bank operations is always related to this. He emphasised throughout—and this is a big step beyond the Currency School position—that there is a dangerously long-time lag between movements in 'bank-money' (i.e. bank deposits) and the corresponding movement in notes, 'so that a control over the volume of notes operates too late—after the evil has been done by a change in the volume of bank money which may have taken place some months earlier'.† He went further and, emphasising the crucial position of the rate of interest in the equilibrating mechanism, urged central bankers to think more directly in terms of forcing the rate of interest to behave in the required way.‡

This was of course not the end of the Keynes story, and in the *General Theory* (1936) Keynes developed ideas that drastically modified the picture of the economic system as a self-stabilising system. Particularly, he argued certain consequences of the shortcomings of the rate of interest as part of the equilibrating mechanism: the liquidity trap and the notion of an underemployment equilibrium came in. Like the *Treatise* before it, the *General Theory* bristled with ideas, many of which were hotly disputed; but both in his 1930 emphasis on the rate of interest as a crucial cog in the mechanism and in his subsequent relative loss of faith in its efficacy Keynes had wide following. In such investigations as they were able to make, the Radcliffe Committee found a good deal of support for the later Keynesian view. In this sense the scepticism of Radcliffe is Keynesian, but in certain other respects—and here I am thinking of parts of the *Treatise* that Keynes did not revise—the Radcliffe view of the economic system is quite unlike that of Keynes.

The break from the Keynesian exposition may perhaps best be seen by exaggerating it, imputing to Keynes a simplicity that was never his. Keynes was thoroughly traditional in drawing a sharp line between money and other assets. To him money had changed from being coin and notes to being notes and bank deposits, notes being the small change of 'bank money'. In his model, there are four major classes of assets: cash, bank money, bonds, and others; there are central banks (whose liabilities are cash), there are banks (whose liabilities are bank money and who are holders of cash and bonds as well as other assets) and there are non-banks (i.e. corporations and individuals who are holders of cash, bank money, bonds and other assets, and who are also much concerned with spending on 'real' assets). A distinction between money and bonds is that the former is useful for payments and the latter are not; another distinction between them is that bonds do and money does not carry interest. With this classification and some reliance upon the interest rate as a balancer of

* Cf. J. R. Hicks, 'A Suggestion for Simplifying the Theory of Money, *Economica*, Feb. 1935, p. 3.

† Vol. II, p. 264.

‡ Vol. II, pp. 369–74; cf. Report of the Committee on Finance and Industry ('Macmillan Report'), Cmd. 3897 (1931), para. 359.

savings and investment, we can make a coherent working model, useful for explaining how policy should be shaped.

But the world in which we live lacks these sharp distinctions. And, remembering that for purposes of policy we must get as close as we can to the points of business decision, the more we try to eliminate dangerous time-lags, the more blurred the distinctions become. There is no clear line between purchasing power that carries no interest and interest-earning assets that have no influence on purchasing power. 'Commercial banks' shade into industrial banks, savings banks and building societies, and these into a host of other financial intermediaries; the liabilities of these are close substitutes for each other, so that a clamping down on one group will not create such an abrupt scarcity of liquidity as will have a worthwhile impact on the pressure of total demand. This is, of course, the kind of argument that was being used by the Banking School a hundred years ago, when J.S. Mill and others pointed out that the substitutability of bank deposits for notes was high, whence they argued that a control of notes alone would be merely a nuisance. Time's answer to the Banking School was the recognition that bank deposits were as good as notes, coupled with institutional arrangements for making good a Currency School contention that the supply of bank deposits was proportional to the supply of notes. Having got so far, we then got into the habit of saying that bank deposits were the important quantity, that our central banks should regulate the bank deposits, and that notes could be left to take care of themselves.

Can we now, as a latter-day Currency School, argue that, although other financial claims are good substitutes for money, we can rely upon the constancy of their ratio to the bank deposits we have learned to control? I doubt it. Or can we now, following those who taught us all about central banking, force the non-bank lenders into a pattern by imposing on them liquidity rules analogous to those whereby the clearing banks are controlled? The Radcliffe Committee looked at the question, and shuddered. Or can we try to check the development of financial institutions, by proclaiming that none but the clearing banks may create money? This is not just an idle fancy: our ancestors did something like this when they strangled the country note issue—but they did not stop the private creation of money. They could not stop it, because money is the creation of the public that chooses to impute certain qualities to certain claims.

I have referred repeatedly to the Currency School and Banking School controversy because I believe that the arguments of the last few years have very close parallels with that earlier controversy. The Currency School case depended on sharp lines of distinction and rigid proportionalities. The case was accepted as the basis for practical policy; we muddled through the next hundred years partly modifying the policy, partly by imposing proportionalities, and partly enduring disadvantages we no longer find tolerable. In modification of the policy, we developed central banking to dull the terror of financial crises, and now we have gone much further by insisting that there shall be no trade depression. The age-old tendency of financial institutions to become more numerous, more varied, and more efficient, flourishes in an economy that avoids major breakdowns; the process is accelerated, and a policy that depends on a stable relationship between institutions becomes less and less realistic.

This is a negative conclusion, and it is because they took this view of the nature of money and the monetary processes that the Radcliffe Committee's conclusions appeared to give such little positive guidance. But even negative conclusions have their importance: we sometimes need to search around for something quite different which will get results. And the conclusions to which our general discussion points do, after all, have highly practical implications. To take a major point, restriction of the supply of bank deposits, if long continued, can be expected to become increasingly ineffective as a curb on total demand, because the demand for liquidity can be so well satisfied from other sources. Worse than this, restriction of the banks will increase the opportunities for alternative lenders, so that the banks will lose ground to their competitors and the heads the government can hit will become progressively less influential in the behaviour of the economy. Or to take another point, on which the Radcliffe Committee's recommendation was grossly misrepresented in some quarters, consider the channels for local authority borrowing. The fact that Treasury bills are, and local authority mortgages are not, 'liquid assets' to the clearing banks has some very short period relevance, in that

the banks are outstandingly efficient lenders, but if the market for credit is fundamentally a single market, an addition of short government paper has much the same effect on the monetary situation wherever it is injected. Banks are not the only lenders sensitive to liquidity.

Our analysis has its uses for bankers as well as for policy-makers. The English clearing banks, acting as a cartel, fix the rate they pay on time deposits, and have only one class of such deposits: at seven days' notice. They are sometimes urged to compete for deposits now going to building societies or hire-purchase finance houses by offering higher rates for longer periods. Against this proposal, it has been argued that the total of deposits in the clearing banks is fixed by the supply of 'liquid assets'. and that therefore the banks could get no more deposits by offering higher rates but would simply reduce their own profits. This over-simple argument can be made true only if we make certain assumptions about strictly rigid proportionality in the balance-sheets of all sorts of firms and households, and insist that for one size of the national debt and one size of the national income there is only one possible size for the total of bank deposits. This is a particular manifestation of the old idea that there is only one kind of money (in this case bank deposits) and that the demand for it is rigidly related to the national income. But once we admit that the liabilities of other institutions may be quite good substitutes for the liabilities of the clearing banks, and that business attracted by non-bank intermediaries affects the non-bank demand for national debt, the consequences of non-competition by the clearing banks become expensive to them. By refusing to compete, they stand to lose a larger and larger proportion of the total lending business in the country. To put the point in an extreme way, if the banks always ignored competition on the principle to which I have referred, their places as creators of the chief monetary assets would gradually be lost to other more aggressive financial intermediaries. And no doubt there would be quantity theorists a century hence to point out that the then predominant money (claims on the aggressive intermediaries) had increased roughly in proportion to the national income: they might even find that clearing bank deposits had become the small change of the monetary system!

Or are we perhaps already there? If the crucial decisions to place orders for production depend 'upon the composition of the spender's assets and on his borrowing power and . . . upon the methods, moods and resources of financial institutions and other firms which are prepared (on terms) to finance other people's spending',* then in an important sense bank deposits have already become the small change of the system. This is no fad of a theorist's exposition, for its implications are surely lessons of the first importance to practical policy. For it means that there is no dependable monetary ceiling against which a boom will bump its head. As the boom rises, the sources of liquidity broaden, and the rising demand for the means of payment comes too late and is too easily satisfied. It follows that a policy of stabilising bank deposits and waiting for the expanding volume of payments to force interest rates upward, while intuitive central bankers try to be nimble in 'following the market', is not enough; action would always be too late, as it was when strain on the note-issue was expected to set the corrective mechanism in motion. If monetary policy is to be used at all, we must make it operate on as broad a front as possible; action on the supply of bank money must be thought of as purely incidental to a positive, aggressive and far-reaching interest-rate policy. How far we can depend on this is, as the Radcliffe Committee emphasised,† a matter on which much more systematic investigation is required. If we want to catch up with events and accept the implications of Full Employment policy, we must urgently investigate the efficacy of a far more vigorous interest-rate policy than any we have yet seen, and, lest this should (as I expect) prove a non-starter, we must think again on how the flexibility of fiscal policy might be increased.

* Radcliffe Report (Cmd. 827), para. 389.
† Ibid, para. 474.

THE PLEISTOCENE HISTORY OF THE IRISH SEA*

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In Great Britain the glacial deposits of East Anglia are the corner-stone on which the Pleistocene history of the country is founded. In Ireland the deposits of the south-east—Cork, Waterford, Wexford and Wicklow—give the corresponding foundation, and this attempted reconstruction of events in the basin of the Irish Sea during the Pleistocene Epoch is essentially built outwards and upwards from south-east Ireland. The table of correlations (Fig. 1) is an extension and development of an earlier table (Mitchell, 1957).

In the British Isles the only coherent record of the Lower Pleistocene is in the Crag deposits in East Anglia, where the opening of the Middle Pleistocene is also well recorded in the Cromer Forest Bed Series. At Cromer we have a modern flora without Tertiary relicts (though the fauna has a more antiquated appearance), and an interglacial sea-level, which approximates to the level of the present day, not to the high levels postulated for the Tertiary erosion platforms, or for the earlier stages of the Pleistocene.

Geomorphologists argue (see Davies, 1960) that the basin of the Irish Sea was formed in the Tertiary Epoch. There is some evidence that the final details of the basin were only completed in the Lower Pleistocene.

Lower Pleistocene

Red Crag shells have long been known from the glacial deposits of the Isle of Man (Lamplugh, 1903). N. Fisher McMillan (1938) has described a rich assemblage of re-deposited Red Crag shells collected from outwash gravel at Killincarrig, Co. Wicklow. The undamaged state of many of the shells indicates that they cannot have been transported far, and therefore in Red Crag times marine shells accumulated in the northern half of the Irish Sea basin. Wirtz (1953, p. 272) notes that the Killincarrig fauna has a marked northern aspect, and concludes that the sea in which the shells lived had no connection with southern waters. If he is correct, then a ridge of high ground, dividing the Irish Sea into northern and southern bays, must still have survived as late as the Red Crag stage.

Wirtz (*l.c.*, p. 272) returns to this point in his discussion of the St. Erth beds, whose fauna he regards as a typical Atlantic–Mediterranean Lower Pleistocene fauna without any Boreal component. Like Kendall and Bell (1886), he considers that a barrier which excluded northern forms still lay across the Irish Sea when these beds were accumulating. Wirtz regards the St. Erth fauna as without doubt younger than the Red Crag fauna of the northern part of the Irish Sea basin, and places it in the Cromer Warm Period. But in my opinion the sea level recorded at St. Erth is far too high for the Cromer Period, and I am compelled to place the St. Erth beds in a warm period between the Red Crag and the Cromer Series. Two such periods, the Waal Warm Period and the Tegelen Warm Period, are well known in Holland (see Woldstedt, 1958, p. 70).

The St. Erth beds lie at about 100 ft., and while the contained fauna required some depth of water for its existence, it seems unlikely that Reid's claim (1890, p. 65) for a sea-level at 340 ft. can be sustained. A high-level gravel deposit at Hele, near Barnstaple, suggests Lower Pleistocene aggradation to about 185 ft., and a similar deposit on the Scilly Isles indicates a level of the same order. Bell's view (1887) that the St. Erth beds record a sea-level of about 200 ft. may well have been a shrewd one.

The village of Hele lies at an altitude of 125 ft. one mile south-west of Barnstaple in Devon;

* Presidential Address delivered to Section C (Geology) on September 6, 1960, at the Cardiff Meeting of the British Association.

Fremington is two miles farther west. Immediately south-east of Hele there is a conical hill of gravel rising to 185 ft. Gravel also occurs at Ellerslie, three-quarters of a mile west of Hele at 125 ft. The gravel rests on a rock-ridge which later became for a time an island, because the ridge is surrounded by a shore-platform cut in a later interglacial period. This high-level gravel is fine-grained, contains flint and weathered rock (both local rock and rock with tourmaline), and is much disturbed by frost-action. It indicates, first, aggradation to at least 185 ft. and, second, that since its deposition neither ice nor sea-level are likely to have attained this height. At Fremington both boulder clay and beach deposits occur at lower levels.

A similar gravel is found in the Scilly Islands, which, except for a small patch of tourmalinised schist (killas), are composed of granite. On the highest part of St. Martin's, at about 150 ft., there is a patch of sub-angular gravel of flint and greensand chert, described by the *Memoir* (Barrow, 1906, p. 15) as 'perhaps of Eocene date', and by Dollar (1957) as '? Pliocene gravel'. Elsewhere pebbles of flint, greensand chert, sandstone, killas, other metamorphic rocks and fine-grained green igneous rocks occur at lower levels. The *Memoir* refers to this material as a glacial deposit, transported by pack-ice, but as we shall see in the next section, a glacial sea with floating ice could not have stood at this level. The material has been disturbed by frost-action, for the *Memoir* (p. 27) speaks of the stones often occurring 'in lenticular patches', and cryoturbation can be seen in Pl. VI. There must have been aggradation deposits to at least 150 ft. on the Scilly Isles, and rivers

	E. BRITAIN	S.W. BRITAIN	WALES AND N.W. BRITAIN	E. IRELAND
UPPER	*Recent*	*Recent*	*Recent*	*Recent*
	Solifluction-earth	Solifluction-earth	Menteith moraine	Solifluction-earth
	Neasham mud	*Hawk's Tor peat*	*Garscadden mud, Paisley marine clay*	*Ballybetagh mud*
	Lake-clay	Solifluction-earth	Shrewsbury, Clyde and Aberdeen moraines	Armoy moraine
	Nazeing arctic peat			
	Escrick, Flamborough moraines		Smestow, Llandaff moraines	Tipperary, Athdown and Killumney moraines
			Chelford mud	
	Hunstanton boulder clay?		Adswood boulder clay	
	Ipswich mud	*Selsey brackish clay*	*Llansantffraid soil*	*Ardcavan, mud*
	Gipping boulder clay	Solifluction-earth Fremington boulder clay	Pencoed boulder clay Fremington boulder clay New Quay boulder clay Solifluction-earth	Brittas, Garryvoe boulder clays Ballycroneen boulder clay Enniskerry boulder clay
MIDDLE	*Hoxne mud* *Kirmington marine clay*	*Fremington beach*	*Gower beach*	*Kilbeg, Newtown muds* *Courtmacsherry beach*
	Lowestoft boulder clay	Porthleven erratics	Gower erratics	Courtmacsherry erratics
	Cromer Forest Bed Series	*Fremington shore-platform*	*Gower shore-platform*	*Courtmacsherry shore-platform*
LOWER	Red Crag and higher crags (recording climatic changes)	St. Erth beds, Hele and Scilly gravels		Killincarrig Crag (re-deposited)

Fig. 1. A correlation-table of the Pleistocene deposits of the Irish Sea.

must have transported the material. In the lower Pleistocene the islands must have been more extensive than they are today, and were perhaps still connected to the mainland. The later shore-platform shows that by the Middle Pleistocene the islands were detached and surrounded by water.

Thus in south-west England we appear to have at St. Erth, Hele and the Scilly Isles water-laid deposits of Lower Pleistocene age indicating a sea-level of about 200 ft.

Middle Pleistocene

CROMER WARM PERIOD: sea-level + 20 ft.; Fig. 2

As noted above, we have at Cromer fossils of modern aspect and a sea-level not very different to that of the present day.

At the end of the nineteenth century a shore-platform a few feet above modern sea-level was described in north-west France (cf. Barrois, 1877). At many places beach deposits with erratic pebbles lay on the shore-platform; floating ice was invoked for the transport of the erratics. In 1904 Wright and Muff described a similar shore-platform at Courtmacsherry and other places in the south-east of Ireland, and this platform (and the cliff against which it abuts) has since been shown to be widely developed. Stephens (1957) has traced the platform along much of the western shore of the Irish Sea. It occurs most commonly about 18 ft. over mean sea-level, ranging from 5 ft. to 25 ft.; it has no discernible tilt. It has not been traced in such detail in Britain, but has been recorded at many localities all round the coast. I do not hesitate to identify the platform at Sewerby in eastern England, and at Brighton and other localities on the south coast with the platform of the Irish Sea. In view of the wide distribution of the platform, its uniformity of level, and the fact that it is developed inside bays (e.g. Cork Harbour) as well as on open coasts, I picture a long period during which the coast-line of the British Isles was essentially the same as it is today, and the level of the inter-glacial sea underwent little change. When was this period?

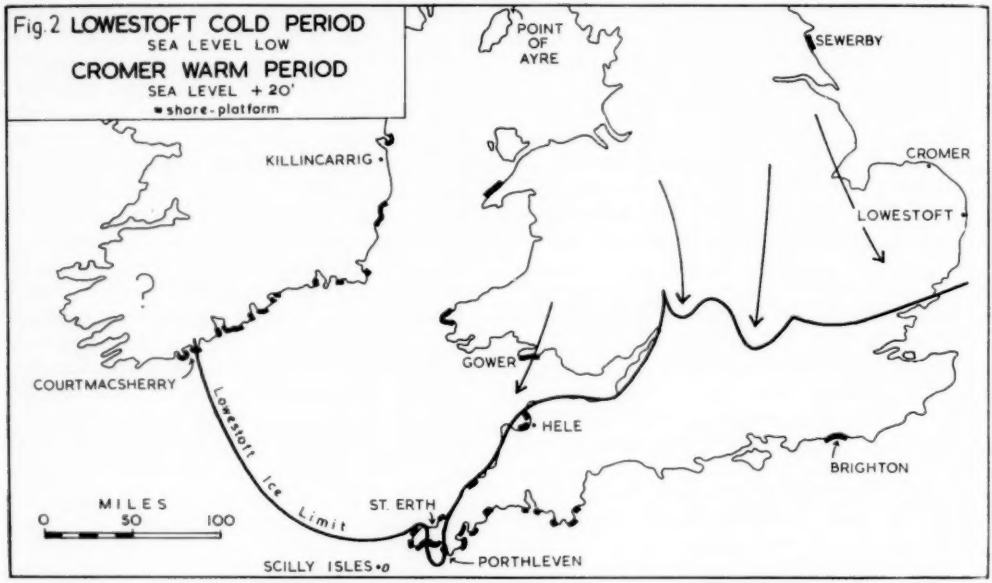
At many places in the south of the Irish Sea (as in north-west France, Cornwall and Devon) beach deposits rest on the shore-platform, and Wright and Muff assumed that the platform and the beach both belonged to the same marine cycle, the platform being cut at the time of maximum submergence, and the beach deposits stranded on it when sea-level fell due to the growth of the ice-masses of the succeeding glacial stage; the erratics were introduced by ice-floes. But a moment's thought will show that the high-level waters of warm inter-glacial seas could not have brought ice-floes to Cork, to Cornwall or to Brittany, and that the cold glacial seas with their floating pack-ice must have had their shore-lines far below the level at which the erratics occur. Ice-floes might carry material along a shore-line; they could not transport material seawards from the land. George (1932, p. 295) noted that many of the erratics in the Gower raised beach came from the hinterland to the north, and 'can only owe their presence to overland transport, of which the sole possible agent is ice'. I attribute the transport of the erratics now found on the platform and in the beaches to an ice-sheet of Lowestoft age that came down the Irish Sea, reaching at least to Cork and to Cornwall.

I place the cutting of the shore-platform seen at Courtmacsherry, Fremington, Gower, the Scilly Isles and many other places in the Cromer Warm Period, and I believe that the Irish Sea of that period was essentially the sea we know today.

LOWESTOFT COLD PERIOD: sea-level low; Fig. 2

From the work of Wills (1950), Arkell (1947), Shotton (1953) and West and Donner (1956), we have a picture of an ice-sheet of Lowestoft age advancing as far as the Thames Valley, Oxford and Gloucester. I extend this picture by postulating a corresponding advance of ice down the Irish Sea at least as far as Porthleven in Cornwall and Courtmacsherry in Cork. No coherent deposits of such an ice-sheet have been identified on the shores of the Irish Sea,* but in my opinion all the erratics—both the large ones which now lie on the shore-platform cut in the preceding warm period, and the small ones which are now incorporated in the beaches deposited in the succeeding warm period—are derived from the glacial deposits of this cold

* A deposit at Nemestown, Co. Wexford, has been regarded as an early glacial deposit (see Woldstedt, 1958, p. 95). A recent exposure shows it to be a periglacial deposit of early Gipping age, resting on the Courtmacsherry raised beach, and covered by Ballycroneen boulder clay.



period. When the ice stood so far south, the sea was much below its present level. Ice-floes may have stranded erratics on the margin of the low-level sea in the Channel approaches. But I know of no agency which could raise the Giant's Rock and the other large erratics at Porthleven from the floor of the English Channel to their present position on the modern coast. The Giant's Rock is a fifty-ton block of garnetiferous microcline gneiss (Flett, 1946). The ice does not appear to have reached the Scilly Isles as the beach there does not contain erratics (Barrow, *l.c.*, p. 16). The lowest boulder clay encountered in the Point of Ayre boring in the Isle of Man may be a deposit of this early *mer-de-glace* in the Irish Sea (see Wirtz, *l.c.*, Fig. 1).

If we consider that the general picture during the Lowestoft Cold Period was the same as the general picture in later cold periods, then a component of this Irish Sea ice will have come from Scotland, and some of the Scottish erratics found in later drifts may be derived from deposits of this early glaciation, just as the Scandinavian erratics in eastern England appear to derive from some very early glacial phase. George, as we have seen, has hinted at an ice-mass of local origin in the mountains of Wales. There should have been corresponding ice-masses on the mountains of the Lake District and in south-west Ireland. The apparent absence of foreign erratics west of Courtmacsherry may indicate that this part of the coast was occupied by local ice.

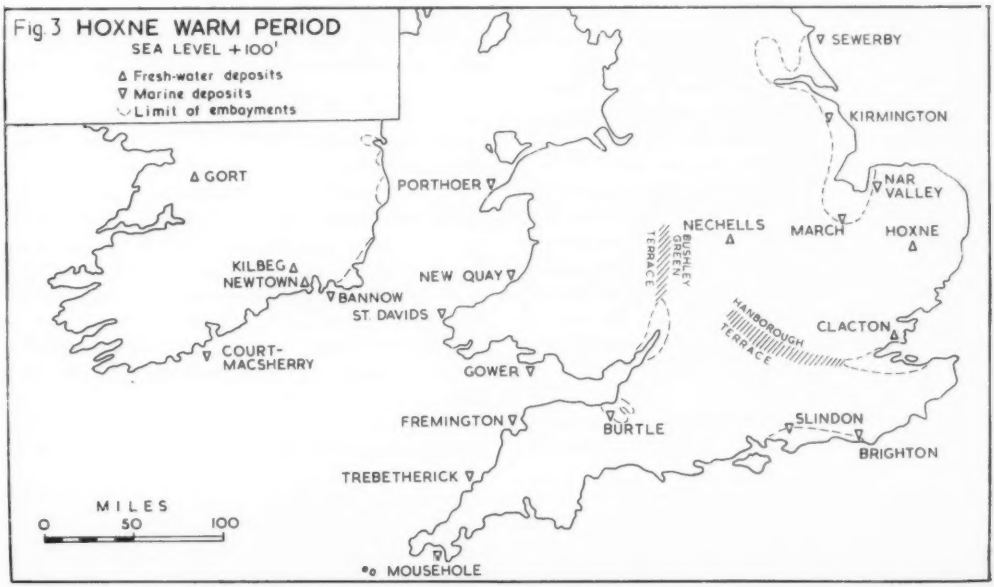
HOXNE WARM PERIOD: sea-level + 100 ft; Fig. 3

We now enter the Hoxne Warm Period, about whose climatic conditions we have reasonably detailed information, thanks to the work of West at Hoxne (1956), of Jessen and others at Gort (1959), and of Watts at Kilbeg and Newtown (1959). At Gort the base of the deposit records sub-arctic conditions, and here we have evidence of the refrigeration of western Ireland during the preceding Lowestoft Cold Period. Kilbeg lies nearer to the Irish Sea, and here the deposit records a warm hyper-oceanic climate, perhaps a little warmer than the warmest phase of the post-glacial. In addition to eu-oceanic plants, there is a central European and also a Caucasian element. In the middle of the inter-glacial period we can picture the west shore of the Irish Sea clothed in an evergreen forest with Fir, Spruce, Pine, Yew,

Rhododendron and Box. Deciduous trees, with the exception of Alder and Birch, were absent. Newly discovered deposits at Baggotstown and Kildrommin are of Hoxne age, and rest on boulder clay presumably Lowestoft in age.

What of sea-level? I envisage that in this interglacial period the earth's surface carried much less ice than exists at the present day, and that sea-level round the British Isles stood 100 ft. above its present level. Marine deposits of this age in the Nar Valley are at 75 ft., and imply a sea-level of not less than 90 ft. (Stevens, 1960); brackish-water deposits lie at 80 ft. at Kirmington (Watts, 1959); though at a lower level, the March gravels also give evidence of marine transgression (see Woldstedt, 1958, p. 87). The Swanscombe terrace of the Thames lies at about 100 ft. Strand-line gravels at about 100 ft. in the Vale of York (Edwards, 1936) may well be of this age. At Fremington, on the south shore of Barnstaple Bay, the shore-platform which was cut in the Cromer Warm Period lies buried below erratic-containing beach gravels which rise to 60 ft. (Fig. 5). Many years ago Sedgwick and Murchison (1840) recorded similar deposits rising to 60–70 ft. at Baggy Point on the north side of Barnstaple Bay. The gravels at Trebetherick lie at 55 ft. (Arkell, 1943). The Burtle Beds (Bulleid and Jackson, 1937) lie between +50 ft. and −35 ft. Sea-level is not likely to have been higher than 125 ft., or else the Lower Pleistocene high-level gravels at Hele and the Scilly Isles would have been washed away. The deposits near Slindon, where heights up to 130 ft. are recorded (Fowler, 1932) may represent a storm beach comparable to the modern Chesil Bank (see Zeuner, 1959, p. 202).

I form the impression that sea-level rose slowly, and perhaps with fluctuations, from its preceding glacial low level. As it rose it washed away the Lowestoft glacial deposits, and the contained erratics were set free to find a new resting-place in the beach gravels. The sea attained the level of the shore-platform cut during the Cromer Warm Period, and continuing to rise left the platform deeply submerged below its waters. The waves that were building up a spit of gravel to a height of 60 ft. at Fremington cannot have been simultaneously either cutting, or cutting away and destroying, a shore-platform 40 ft. below sea-level. The sea continued to rise banking shore deposits against the old cliff, and depositing clays and sands where suitable basins presented



themselves. Having attained its maximum during the warmest phase of the inter-glacial, sea-level began to fall, and again I feel that the fall was interrupted by fluctuations. In such fluctuations we may find explanation for the anomalous level at which the Clacton deposit (Pike and Godwin, 1953) lies. The notches and associated beach deposits near Bristol (MacInnes and Whittard, 1955) and the emerged shore-lines of the Scilly Isles (Dollar, *l.c.*) may have formed during periods when for a time the level remained constant. As the waves fell back to lower and lower levels, they tended to wash away the deposits they had laid down as sea-level rose. At Fremington, in the sheltered waters of Barnstaple Bay, a spit survived more or less intact, at Mousehole a veneer of pebbles was left straggling down the hill-slope at about 65 ft. (Reid and Flett, 1907, Pl. IV), at Sewerby, at Gower and at Courtmacsherry the destruction of the beach deposits was almost complete, save that remnants of beach survived where they rested in the nick where the old shore-platform met the old cliff, just as placer gold is trapped in the riffles on the stream-bed. We can trace the fall of the sea below its present level, because George (1932, pp. 302–8) has described patches of beach gravel cemented to the platform at Heatherslade in Gower at modern low-water mark, and similar patches occur at the same level on the platform in Waterford and Wexford.

At Newtown, Co. Waterford, dune-slacks formed on the beach as sea-level fell, and a thin peat on top of the beach but below later glacial deposits records a woodland of Birch, Pine and Willow. This shows that after sea-level had dropped below its present level cooler climatic conditions had broken up the evergreen forest and replaced it by a simpler woodland of more northern aspect. But the sea cannot yet have been cold enough to carry pack-ice, or else woodland could not have grown near its shores. The shelly marine deposit with a boreal fauna which lies at a depth of −200 ft. in the Point of Ayre boring in the Isle of Man (see Wirtz, *l.c.*, Fig. 1) may well indicate a continued fall, both in level and in temperature, as the next glaciation developed.

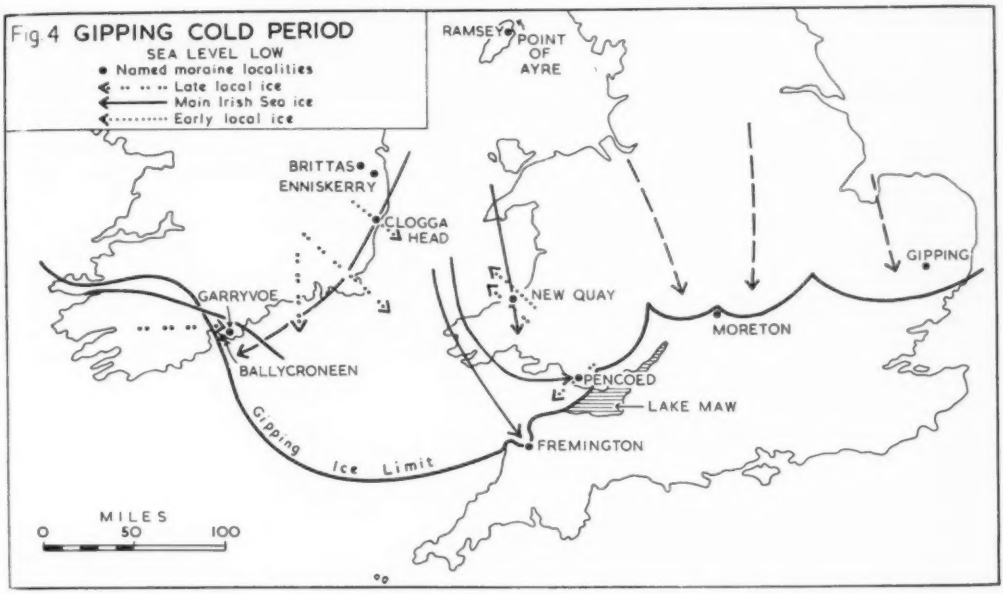
Upper Pleistocene

GIPPING COLD PERIOD: sea-level low; Fig. 4

Ice once more occupied the North Sea basin and sent a stream down eastern England, where it laid the Gipping boulder clay on top of the Hoxne inter-glacial deposit. The Midlands were invaded by eastern ice depositing a chalky boulder clay, and by Welsh ice depositing a boulder clay with western stones. Near Malvern this ice was 300 ft. thick, with its upper surface at 550 ft. (Hey, 1959). The southernmost limit of contact between the two ice-masses was in the vicinity of Moreton-in-the-Marsh. South of the ice outwash gravels built up terraces in the Thames and the Severn valleys.

In Ireland the Gipping Cold Period has three phases which are united into the Munster General Glaciation (Mitchell, 1957). In the first phase ice from the Wicklow Mountains flowed down into the Irish Sea basin. At Clogga Head, Co. Wexford, a stony boulder clay with Leinster granite rests on the shore-platform; Farrington (1954, p. 49) assigns this boulder clay to his Enniskerry Mountain Glaciation. A brown shelly calcareous boulder clay rests on the lower boulder clay. This was deposited in the second phase by a powerful ice-stream which came down the Irish Sea and advanced as far as Ballycroneen Bay in Co. Cork (Wright and Muff, 1904); this second phase is here referred to as the Ballycroneen Glaciation. After its advance, the Ballycroneen ice seems to have weakened, and as it melted back shelly outwash sands and gravels (the Wexford manurial gravels of older authors—see Cole and Hallissy, 1914) were deposited. In the third phase local ice became powerful in the mountains of Wicklow (Brittas Mountain Glaciation) and of Cork and Kerry (Greater Cork-Kerry Glaciation of Farrington, here called the Garryvoe Mountain Glaciation, after the locality described by Farrington—1954, p. 52—where a boulder clay with local rocks rests on sands and boulder clay of Ballycroneen age). In Wexford the Brittas ice laid younger material on top of the deposits of the Ballycroneen ice.

The same three phases are seen on the east coast of the Irish Sea. At New Quay in Cardiganshire a few rounded beach pebbles on the rock surface indicate the former presence of more extensive raised beach deposits corresponding to those at Fremington. Thick stratified solifluction-earth (head) buries the beach and rock. Resting on the head there is a non-calcareous grey boulder clay with local stones and some seams of disturbed gravel; this material, deposited in the first phase



by local ice descending from higher ground, is here called the New Quay Mountain boulder clay. On top of this boulder clay is a brown shelly calcareous boulder clay, the equivalent of the Ballycroneen boulder clay. This boulder clay can also be seen at Pencoed in Glamorganshire (Strahan and Cantrill, 1904, p. 100), and at Fremington in Devon (Maw, 1864); it will here be referred to as the Fremington boulder clay. When the ice of this phase blocked the Bristol Channel, water must have been dammed up in the embayment. Just as we speak of Lake Lapworth and Lake Harrison, I now speak of Lake Maw, and I invite some geologist of Cardiff or Bristol to trace the margins of this lake, and discover the delta that must have been built up where the Severn discharged its load of outwash gravels into it.

I hope in this way to rescue Maw's brilliant work from the neglect it has suffered from some recent workers, who have glibly discussed the Fremington deposits without consulting Maw's paper which appeared in the *Q.J.G.S.* as long ago as 1864. My section (Fig. 5) is essentially the same as Maw's section (his Fig. 3), which shows clearly and correctly that there is no justification for pretending that the beaches at Fremington and at Saunton are stratigraphically above the Fremington boulder clay. The Fremington beach runs below the boulder clay, and if the boulder clay is of Gipping age, the Fremington and Saunton beaches cannot lie in the last inter-glacial period (for the opposite view, see Zeuner, 1959, p. 293).

At the top of the section at New Quay there is a green-grey non-calcareous sandy boulder clay, with many local stones (including some large boulders), deposited in the third phase by local ice descending from higher ground. This material which corresponds with the drift with local stones at Pencoed (which also rests on Fremington boulder clay) is here called the Pencoed Mountain boulder clay. In the Isle of Man a similar three-fold sequence is suggested at Ramsey (Lamplugh, 1903, Fig. 9), and the calcareous boulder clay at −130 ft. to −220 ft. in the Point of Ayre boring is probably the Fremington boulder clay (see Wirtz, *l.c.*, Fig. 1). Two miles north of the town of Cardigan there are thick and extensive deposits of shelly outwash gravel (Williams, 1927, p. 218) derived from the retreating Fremington ice, and corresponding with the Wexford manurial gravel.

It is thus seen that there were complicated ice-movements in the basin of the Irish Sea during the

Gipping Cold Period. First the mountains of Leinster and Wales, and doubtless also the mountains of Scotland, the Lake District, the Isle of Man and of Cork and Kerry, discharged streams of ice on to lower ground. The Scottish stream then gained in strength, and collecting shells and calcareous clay from the sea-floor, flowed on to reach Cork and Devon. This ice then fell back depositing a shelly outwash gravel. Ice from the mountains in and around the Irish Sea then advanced into the basin once more.

On the east coast of Ireland the later ice-streams have removed almost all the beach deposits of Hoxne age. Some beach still survives below boulder clay on the south coast of Ireland, the coasts of Wales and at Fremington. South of the area invaded by Gipping ice frost-action was very severe, and deep deposits of solifluction-earth now mantle the slopes. In Brittany the corresponding head has been traced to 20 m. below modern sea-level (Barrois, 1897). Berthois (1957, p. 552) notes deposits suggestive of Quaternary *limons* at depths of *ca.* 100 m. in the south part of the Irish Sea.

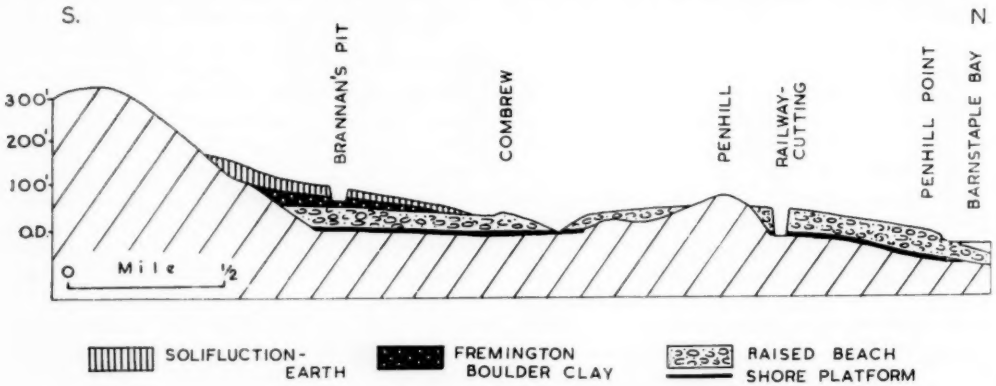
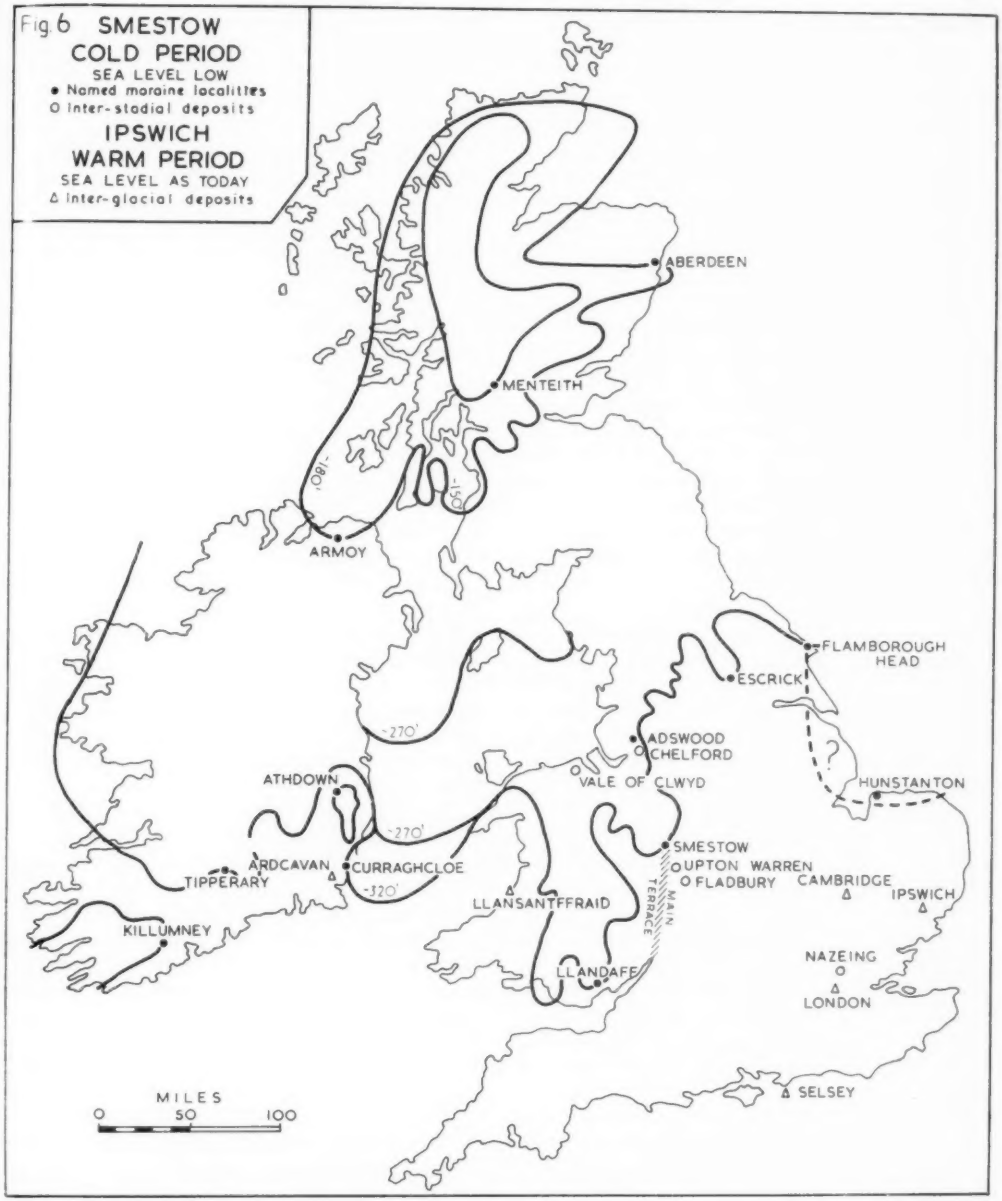


Fig. 5 QUATERNARY DEPOSITS NEAR FREMINGTON, DEVON

IPSWICH WARM PERIOD: sea-level as today; Fig. 6

When the Gipping ice finally retreated from the shores of the Irish Sea, the next inter-glacial period opened. In Denmark, Germany and Holland the development of the woodlands throughout the period is well established, and the fauna of the Eem Sea is well known. It appears that the warmest part of this inter-glacial period was warmer than the post-glacial 'Climatic Optimum'. In England pollen-containing deposits are known at Bobbitshole, Ipswich (West, 1957) Histon Road, Cambridge (Sparks and West, 1959), Trafalgar Square, London (Franks and others, 1958), and at Selsey, Hampshire (West and Sparks, in the press). These deposits are clearly the equivalent of those of the continent. In Ireland fossiliferous deposits are known at Ardcavan, Co. Wexford (Mitchell, 1948). The fresh-water deposit at Ardcavan is small and badly preserved, but does record a change from tundra to temperate woodlands.

In Holland and north Germany the Eem marine deposits are not recorded higher than 7 m. below present sea-level. At Ipswich the fresh-water deposits lie between −10 ft. O.D. and +13 ft. O.D. in an open valley leading to the Orwell estuary and the Thames. At Trafalgar Square the fresh-water peaty silt lies at 0 ft. O.D.; it is covered by sands and silts rising to 30 ft. O.D. in the Upper Flood-Plain terrace of the Thames; the peaty silt shows no marine influence. If the terrace is graded to a sea-level higher than that of today, why is there no marine influence at Ipswich? At Cambridge the fresh-water deposits lie between +10 ft. O.D. and +35 ft. O.D.; there is no positive indication of marine influence. A marine transgression would have no difficulty in reaching the Cambridge region. At Selsey the deposits are estuarine, and are capped by a beach deposit which



rises to 23 ft. O.D. At Ardcavan the deposit is exposed between 6 ft. and 12 ft. above modern high-water mark in a cliff which itself rises to 15 ft. above the same level. Any substantial rise in inter-glacial sea-level must have flooded the basin in which the deposit lay. The Limerick site lies at

300 ft., and will not be considered here. Nor do I consider the beach at Morston, Norfolk (Solomon, 1932), for the age of the beach (which is without fossils) will depend on the age assigned to the overlying Hunstanton boulder clay; the beach is in any case at a low level.

We have thus a series of deposits of Ipswich age, ranging from −10 ft. O.D. at Selsey to +35 ft. O.D. at Cambridge, and only at Selsey is marine influence recorded. Before leaping to the conclusion that the Selsey beach at 23 ft. O.D. must indicate a transgression to that height, we may recall that the modern storm beach known as the Chesil Bank rises to 42 ft. O.D. Apart from the deposits at Selsey, there is in my opinion no clear evidence that sea-level around the British Isles during the Ipswich Warm Period was higher than sea-level at the present day. I believe therefore that during this inter-glacial period the Irish Sea presented the same appearance that it has today. The countryside was covered with woodland of modern aspect, and deep soil profiles developed. Such soils can be seen at Llansantffraid, Cardiganshire, buried by later glacial gravels.

SMESTOW COLD PERIOD: sea-level low; Fig. 6

In the vicinity of Smestow, a village five miles south-west of Wolverhampton, morainic gravels rich in erratics from Scotland and the Lake District grade into an outwash terrace. The terrace follows the valleys of the Smestow and the Stour and grades into the Main Terrace of the Severn (Wills, 1938, pp. 203–6). Thus at Smestow we have the southern limit in the Midlands of the last main advance of ice from the north, and the moraine there forms part of the line that Charlesworth (1928, 1929) and others have traced from the Shannon to the North Sea. My version of the line is shown on Fig. 6, where on the east coast of England the line ends in Flamborough Head (Farrington and Mitchell, 1951). In the Irish Sea I show an asymmetric advance, because the high ground of the Lleyn Peninsula deflected the ice away from Wales and towards Ireland. The submarine contours in St. George's Channel suggest that the end-moraine can be traced from the Lleyn Peninsula to Wexford; in the deepest part of the channel the moraine lies about 320 ft. below sea-level. Recent work by Farrington, Flatres and Synge (unpublished) shows that in western Ireland the ice failed to cover Achill Island and north-west Mayo. There were independent ice-caps on the mountains of Wicklow (Athdown Mountain Glaciation) and of Cork and Kerry (Killumney Mountain Glaciation). The corresponding ice-cap on the Welsh mountains was confluent on the north with the main ice-sheet from the Lleyn Peninsula to Shrewsbury. The free borders reached to Lampeter on the west, to Llandaff on the south and to Hereford on the east; this ice-cap is referred to here as the Llandaff Mountain Glaciation. A further ice-centre in the Lake District (Main Lakeland Glaciation, Trotter and Hollingworth, 1932) contributed ice to the main ice-sheet.

The question immediately poses itself—'Is the outer limit of advance the same age all along the line?' Both in Germany and in the United States the line of farthest advance during the last glaciation is a complex one, made up of a sequence of lobes of different ages.

Water draining away to the south from the ice at Smestow built up the Main Terrace of the Severn. At Upton Warren a thin seam of organic mud is intercalated in the gravel of the Main Terrace, and this has been given a radiocarbon age of 42,000 years: at Fladbury peat below the Avon No. 2 Terrace (which grades into the Main Terrace) has been given an age of 38,000 years (Shotton, 1959).

At Chelford, Cheshire (Simpson and West, 1958) an organic mud (indicating inter-stadial conditions, with wood of Birch, Pine and Spruce) lies below a boulder clay of Smestow age. Radiocarbon dating gives the age of the wood as 57,000 years. Simpson and West equate the Chelford mud with the Brorup inter-stadial deposit in Denmark (dated to 55,000 years, and showing a similar vegetation pattern), and Brorup in turn equates with the Gottweig Inter-stadial Stage of Central Europe. The Main Irish Sea Glaciation of Wills with its outer limit at Smestow should thus be the equivalent of the Main Würm (*Haupt-Würm*) of Europe, and just as the Main Würm succeeds the Aurignacian Oscillation, so the boulder clay of the Smestow glaciation seals the caves of the Vale of Clwyd which contain palaeolithic implements. At Chelford lumps of a lower boulder clay are dredged up from below the mud, and this is equated by

Simpson (1959) with the boulder clay which appears at the surface at Adswood, nine miles north-north-east of Chelford. As the 'Lower Boulder Clay' of the Cheshire Plain is not deeply weathered, the Adswood boulder clay may be early Würm, and all the traditional deposits of the Cheshire Plain—'Upper Boulder Clay, Middle Sands, and Lower Boulder Clay'—may belong to the last glaciation. I cannot support the views of Poole and White (1960).

We have at Tipperary and Curraghcloe in Ireland, at Escrick and at Flamborough Head in Yorkshire, a tumultuous end-moraine which may well be synchronous in these localities and of Main Würm age along its length. At Flamborough Head the moraine now disappears eastward below the waters of the North Sea. What of Holderness and Hunstanton, where Valentin (1957) and West (1957) are proponents of a last glaciation age for the deposits? I cannot agree with Valentin's views. In north Norfolk I must agree with West that apparently young features are to be seen, but at Holkham brick-pit the boulder clay is deeply leached. Perhaps between Flamborough Head and Hunstanton we have deposits and topography of early Würm age, equal in time to the Adswood boulder clay of the Cheshire Plain.

When the ice-front stood at Tipperary, Smestow and Escrick, severe climatic conditions obtained over the rest of the British Isles. Frost-action renewed the process of smoothing the topography and mantling hill-slopes with solifluction-earth. At Upton Warren the organic remains indicate a climate like that of the north Swedish tundra (Shotton, 1959).

From the limit of main advance the ice fell back, and the retreat may have continued, uninterrupted by any major re-advance, until the British Isles were clear or almost clear of ice. Two ridges, perhaps morainic, perhaps dead-ice features, cross the floor of the Irish Sea about 270 ft. below modern sea-level. One runs from Cahore Point in Wicklow to the Lleyn Peninsula; the second runs from the vicinity of Lambay to the Isle of Man and thence to England, though the eastern end of this ridge has now lost its identity in later coastal sands. Deep basins lie between the ridges. A line of moraine shown in Fig. 7 of my 1957 paper running through the town of Kells is now omitted, as Mr. Synge tells me that features formerly mapped as moraine (Synge, 1952) are probably dead-ice features.

In Holland a phase of amelioration of climate allowed the accumulation of organic matter at Breda, and this has been given a radiocarbon age of about 30,000 years, roughly equivalent in time with the Paudorf inter-stadial stage in Austria (Andersen and others, 1960). At Nazeing in the Lea Valley rafts of arctic peat deeply embedded in gravel have been given a radiocarbon age of the same order (Godwin, unpublished). The rafts, which cannot have travelled any great distance, may have been engulfed in gravel set in motion by extensive thaws. Cold conditions then returned in Holland, but whereas before the amelioration the climate was wet as well as cold, after the amelioration the climate was dry and cold, giving rise to extensive formations of coversand. Ice formed again in the British Isles, but on a much smaller scale.

In Scotland ice from the highlands filled the Great Glen, and moved both south-west and north-east. In the south-west the ice crossed the North Channel and mounted on the Irish coast from Ballycastle to north-east Inishowen, building up the Armoy moraine. Between Inishowen and Islay the moraine lies about 180 ft. below modern sea-level; in the Firth of Clyde the corresponding moraine is 150 ft. below sea-level (Ting, 1937). Synge and Stephens (1960) consider that the features mapped as Antrim Coastal Re-Advance south of Fair Head (Charlesworth, 1939) are probably dead-ice features of greater age. North-west of the Great Glen the ice filled the Moray Firth, and advanced east from the Grampians to the coast at Aberdeen (Synge, 1956). The advance from the Welsh mountains that once more brought ice to Shrewsbury (where its outwash built up the Worcester Terrace of the Severn) (Wills, 1937) may be of the same age. Ice again occupied corries and valleys in the mountains of the Lake District and of Ireland.

With the retreat of the ice from the Armoy moraine the late-glacial stage opens and the final disappearance of cold conditions begins. Ice melted away in Scotland, and as the country had been deeply pressed down beneath the weight of the ice, the rising sea quickly flooded the land and deposited a marine clay with arctic shells (Paisley marine clay, Jamieson, 1865, p. 175). The inner

margin of this arctic sea is marked by the 100-ft. late-glacial beach (Donner, 1959).

The well-known temperate interlude in the late-glacial stage in the British Isles (see Godwin, 1956, pp. 15–21), correlated with the Allerød phase in Denmark, has now been dated by Godwin and Willis (1959) to about 9000 B.C., and is clearly later in time than the arctic sea. A minor deterioration of climate brought cold conditions back for the last time; ice from the Highlands advanced again as far as Menteith (Donner, *l.c.*) and high valleys and corries in the mountains of Wales (Seddon, 1957), and doubtless of the Lake District and Ireland also, were partly refilled with ice; feeble solifluction developed on the lowlands where the Arctic Willow (*Salix herbacea*) was widespread. With the return of warmer conditions the Recent Warm Period opened.

RECENT WARM PERIOD

Radiocarbon dates (Godwin and Willis, *l.c.*) show that the post-glacial amelioration of climate, which marks the opening of the Pre-boreal Stage—Zone IV of the pollen-diagrams—began in the British Isles about 8300 B.C. By this time ice had effectively disappeared from Great Britain, but there was still some ice in Scandinavia, and a great deal of ice in North America. Records of plant-fossils show that early in Zone IV warm-loving plants were crowding back into north-west Europe as temperature was no longer a barrier to migration.

We have seen that there are two ridges probably of glacial origin spanning the Irish Sea about 270 ft. below its present level. One ridge runs directly from Wales to Ireland, the second runs to Ireland *via* the Isle of Man. Were these migration-routes still open in the Pre-boreal Stage? The last part of the post-glacial eustatic rise in sea-level came late in north-west Europe. About 7000 B.C., early in the Boreal Stage (Zone V), peat (moor-log) was still forming on the floor of the North Sea, 170 ft. below modern sea-level. Where did the water that flooded the North Sea come from? In Scandinavia the remnants of the ice-mass had already divided near Ragunda; in North America the ice of the Great Lakes region stood at Tolleston and at Lundy (Flint, 1957, pp. 324–7). It must have been the final melting of this American ice-mass (and perhaps other extra-European ice-masses) that raised sea-level by 170 ft. (Farrington, 1945). At the opening of the Pre-boreal Stage the ice in Scandinavia was building up the Mid-Swedish End-Moraines, at Linköping, over three hundred miles south of Ragunda, and the ice in North America stood at Valders, over one hundred and fifty miles south of Tolleston. In melting back from Linköping to Ragunda, and from Valders to Tolleston, enough water must have been set free to raise sea-level at least by 100 ft.

Those, if any, who think that these estimates of post-glacial eustatic rise in sea-level are too great, may note Flint's remarks (*l.c.*, p. 259) on the isostatic adjustment of the sea-floor to the weight of overlying sea-water. A point on the floor of the Irish Sea now 270 ft. below modern sea-level may have stood relatively higher before the floor was carrying an overburden of water.

We may therefore picture that at the beginning of the Recent Warm Period sea-level in the Irish Sea basin was far below its present level, and that it was in the Pre-boreal Stage that the moraine bridges across the Irish Sea were severed. Before this separation took place a large number of animals and plants had opportunity to enter Ireland. If we consider that the link between the Isle of Man and Britain lasted longer (but not much longer) than the link between Ireland and Britain, it might be possible to explain some of the puzzling features of the Manx flora and fauna (cf. Allen and Cowin, 1954).

The North Channel area is more difficult. Just south of Ailsa Craig in the Firth of Clyde and 150 ft. *below* modern sea-level there is a moraine (equivalent to the Armoy moraine) whose topography, pitted with small hollows, shows that it was not deposited under water (Ting, *l.c.*). When the ice withdrew from this moraine the land was depressed to such an extent that the late-glacial sea flooded up against the promontory of Kintyre (which bounds the Firth on the west) rising to a height of 100 ft. *above* modern sea-level (Donner, *l.c.*, p. 11). Therefore at the time of the Paisley marine clay the moraine was at a level 250 ft. below modern sea-level. How far down the ground on which the moraine now rests was when the ice of the last glaciation was at its maximum has yet to be determined. In view of this uncertainty, I will not here discuss the belt of now-submerged moraine of Armoy age that runs from Islay to

Inishowen at a maximum depth of 180 ft. below modern sea-level as a possible migration-route into Ireland in early post-glacial time. Stephens (1958) records evidence of late-glacial high sea-levels in Antrim and Down, and at the time of the Paisley marine clay Ireland must have been separated from Scotland by sea-water. Isostatic recovery of the land may then for a time have outstripped the eustatic rise in sea-level, and Ireland may have been temporarily re-united with Scotland. Rising sea-level then again gained on the rising land, and by 6000 B.C. sea-water was once more in occupation of the North Channel. Beach-gravels began to accumulate in the mouths of the Antrim glens, and at Cushendun mesolithic implements of Larnian type occur in the lower layers of the gravels.

Here the archaeologist may take over the story from the geologist.

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ECHOES from the Past

Organic Chemistry applied to Physiology and Pathology

'. . . In a chemical sense, therefore, it may be considered that a carnivorous animal in taking food feeds upon itself, for its nutriment is identical in composition with its own tissues. . . .'

'How beautifully simple, then, by the aid of these discoveries does nutrition appear! Those vegetable constituents which are used by animals to form blood, contain the essential ingredients of blood ready formed. In point of fact, then, vegetables produce in their organism the blood of all animals; for the carnivora, in consuming the blood and flesh of the graminivora, consume, strictly speaking, the vegetable principles which have served for the nourishment of the latter. In this sense, we may say that the animal organism gives to blood only its form; and further, that it is incapable of forming blood out of other compounds which do not contain the chief ingredients of that fluid.

'Whilst considerations such as these have led Liebig to these conclusions regarding the increase of mass in animals, he has still to account for the use of the substances in food, which are destitute of nitrogen, but which we know are absolutely necessary to animal life. Such substances are starch, sugar, gum and pectine. In all of these substances we find a great excess of carbon, with oxygen and hydrogen in the same proportion as in water. They therefore add an excess of carbon to the nitrogenised constituents of food, and they cannot possibly be employed in the production of blood, because the nitrogenised compounds contained in the food already contain exactly the amount of carbon which is required for the production of fibrine and albumen. By a train of admirable reasoning, Liebig arrives at the interesting conclusion, that they are solely expended in the production of animal heat, being converted by the oxygen of the air into carbonic acid and water. . . . The carbon of the food not meeting with sufficient oxygen to consume it, it passes into other compounds containing a large excess of carbon and deficiency of oxygen, or, in other words, *fat* is produced. It is thus that the sedentary ladies of oriental countries acquire so much embonpoint, and stall-fed animals so much fat. . . .'

Based on report by Professor Liebig, British Association Meeting, 1842.

The British Association

'There are probably few among us of whom the inquiry has not been made after any one of our meetings—whether any striking discovery had been brought forward; and most of us will also probably have remarked that an answer in the negative has frequently produced something like a feeling of disappointment in the inquirer. But such a feeling can only arise from a misapprehension of what I conceive to be the real and legitimate objects of the British Association. Great discoveries do not require associations to proclaim them to the world. They proclaim themselves. We do not meet to receive their announcement, or make a display of our scientific labours in the eyes of the world, or to compliment each other on the success we may have met with. Outward display belongs not to the proceedings, and the language of mutual compliment belongs not to the language of earnest-minded men. We meet, Gentlemen, if I comprehend our purpose rightly, to assist and encourage each other in the performance of the laborious daily tasks of detailed scientific investigation.'

British Association Report, 1853.

* * *

SCIENCE BROADCASTING IN SOUND AND TELEVISION*

By A. B. OLDFIELD DAVIES, C.B.E. *Controller, Wales, British Broadcasting Corporation*

May I begin by calling your attention to something which Lady Albemarle said in her Presidential address to this Section last year:

> I think it is true to say that the B.B.C. has discharged its responsibilities in this field (i.e. in teaching us to live with science) in a substantial and imaginative way. . . . Many of its programmes on sound and television have sparked off interests.

After giving some examples, Lady Albemarle went on to observe:

> Having paid honour where honour is due, I must now say that I think there is a lot still to be attempted.

If I may, I should like to take this as a starting point for some reflections on Science Broadcasting in Sound and Television—making clear at the same time that having been for many years a B.B.C. administrator and not a producer I can claim no personal share or even first-hand knowledge of this work, but I regard the invitation to preside over Section X this year as a compliment to the work of the B.B.C. in this field.

To some extent science broadcasting can be regarded as an ally, so to speak, of the British Association for the Advancement of Science. The word Advancement has at least two connotations—first the extension of scientific knowledge by the encouragement and protection of scientists and by the promotion of particular projects, and second, there is advancement in the sense of making the scientific knowledge already acquired more widely known, even the popularisation of science. It would appear to me that in recent years the latter rather than the former has been regarded for the time being as the primary task of this Association.

* Address delivered to The Assembly of Corresponding Societies on September 1, 1960, at the Cardiff Meeting of the British Association.

In this work it has allies and collaborators, and it will be part of my task to show that the B.B.C. has been among them. In an important sense the act of broadcasting and of listening or viewing is itself an acknowledgement of the tremendous debt we owe to scientific advances, since it is an outstanding example of the application of scientific discovery to the satisfying of the human need for communication. One of the great scientific wonders of the age has become a normal article of domestic equipment, though the most that many of us know about it is how to switch on and switch off. It is a mass medium in the sense that what is communicated is *available* to all. It sometimes happens that nearly everybody in the country is looking at or listening to the same thing, as at the Coronation or a royal wedding. But a mass medium does not necessarily imply a mass audience. To attain any audience at all the broadcaster must decide as clearly as he can whom he wants to address, whom he is attempting to communicate with. What is the target? Some broad categorisation of the audience is necessary or a broadcaster and producer will shoot many arrows into the air and be as ignorant of where they fall as the singer in the song. Any statement of the intention of a broadcast programme starts with an assumption about the nature of the audience to be reached. In schools broadcasts and schools television the programme planners can state definitely that the intention is to cater, say, for the thirteen-year-olds in secondary modern schools—they may even go further and specify rural schools. There you have a broad category and certain assumptions can be made about the particular audience which will help in planning the programme. In schools programmes, too, the broadcaster can rest secure in the knowledge that there are teachers in charge of groups of listeners

and viewers who are going to drive home his points or use the programme for their own particular teaching purposes. But when we come to programmes for adults the task of defining intention, of describing the target and hitting it, is much more difficult. For instance, it is important to decide whether one is seeking to entertain or to inform—and if a bit of both, in what proportions and which comes first, the sugar or the pill. Too little sugar makes the pill unpalatable: too much may make it ineffective. There is value in apt illustrative material, but there is also the danger of the illustration itself being remembered at the expense of the point which it seeks to illustrate. We have all at some time or other attended popular science lectures when we have been enthralled by simple experiments and have tended to regard the lecturer as a species of clever conjurer but have managed to miss the point of most of the experiments. The amount of sugar in the pill has to be carefully assessed, and the proportion will vary in accordance with the nature of the audience. I suppose it is the pill, the whole pill, and nothing but the pill when broadcasting offers an opportunity for experts to communicate with experts. The point was forcefully made by the President of this Section at the Bristol meeting of the British Association in 1955 that, as a consequence of intensified specialisation with its accompanying use of technical terms, with each branch and sub-branch of science tending to develop a jargon of its own it was increasingly difficult for scientists to speak even to scientists.

How much more difficult, and in the view of many people more vitally necessary, is the task of broadcasting science to people who have no scientific training. They can be very broadly divided into those whose scientific education stopped at O Level chemistry, physics, mathematics or biology, and who went on with their education, if at all, either in pursuit of Arts courses at Advanced and University level or in acquiring professional or technical qualifications to fit them for their subsequent careers. Then there are those who leave school at fifteen and whose formal education is virtually over at that stage. These latter constitute the great majority—these are the potential mass audience. It seems to me that it is in broadcasting through Sound and Television to the non-scientific but literate people and to the largely non-educated or partially educated masses that it is important for science broadcasters to be clear as to their intentions and aims.

This means considerably more than bridging the gap between what Sir Charles Snow has called the two cultures:

> Literary intellectuals at one pole—and at the other scientists, and as the most representative the physical scientists. Between the two a gulf of mutual incomprehension—sometimes (particularly among the young) hostility and dislike, but most of all lack of understanding. They have a curious distorted image of each other.*

Whether or not one accepts the thesis presented by Sir Charles Snow who, with a foot in both of the camps that he describes, speaks with all the greater authority, it is relevant to my theme to inquire to what extent scientific culture can be presented through broadcasting not only to those who lack science but to those who lack interest in or taste for culture in any form. It is to be hoped that bridging the gap does not mean that non-scientists must become scientists. It is not so much the formal acquiring by non-scientists of a body of scientific knowledge as developing in them an attitude of positive, sympathetic, active and friendly interest in scientific matters, a respect for scientific method and achievement, and an awareness of the significance of scientific advances. Not so much straightforward instruction as an acceptance of the fact that scientific knowledge is related to social needs and conditions. And when it is remembered that more and more scientific research, particularly pure research, is financed directly or indirectly from public funds and that the ultimate authority for such expenditure rests with us all—with the people, whatever our level of culture—it is all the more important that the mood of the people is favourable; that the climate of opinion is such as to promote and not discourage public spending on scientific research. This is something which broadcasting can help to create, not merely by programmes explaining in broad, popular terms what is being attempted and achieved, but also by introducing scientific leaders to the general public to a much greater extent than was ever possible in pre-broadcasting days. Familiarity with the voices, accents, speech styles of leading scientists, and in television their appearance, mannerisms, intimate tricks of twitch-

* C. P. Snow: *The Two Cultures and the Scientific Revolution*, p. 4. The Rede Lecture 1959 (C.U.P.).

ing the nose or ruffling the hair, help to establish scientists as human beings and not as lords of creation. They need not be Olympian figures, remote and massive in their high distinction, but men and women who talk, smile, argue, become irritable, are humanly susceptible to a little flattery, are humble and vain, pleased by some things, perplexed by others—in many ways appearing to be just like ourselves except that they have a stronger sense of purpose, an air sometimes of aggressive dedication, and that they know far more than we do. Let me take as one example, Lord Russell, who through radio and television has become a 'familiar' figure. I don't know to what extent his ideas are better known, more deeply appreciated, more thoroughly understood and more widely accepted than they would have been had our opportunities of contact with him been confined to reading his books or listening to an occasional lecture in some public hall. What I am sure of is that as a person Lord Russell is appreciated to an extent that would never have been possible had he not appeared in radio and television. But there is more to broadcasting than the presentation of acceptable images of distinguished scientists by giving them opportunities to project their personalities on the air.

Let us take sound broadcasting first. It has some substantial advantages over television. The most obvious is that with the present provision of sound broadcasting in this country under one management so to speak, there is greater flexibility and more variety of programme services than is possible in television at the present time.

Of the four domestic programme services—Home, Light, Third and Network Three, with regional variations in the Home Service—the Third Programme is the obvious vehicle for statements that are authoritative, learned and expert, making few concessions to the frailty of listeners, though the Reith Lectures get their first hearing in the Home Service. But at a different level of understanding much elementary scientific information can be conveyed in programmes that are not directly scientific in intention. This is true of television as well as of sound radio programmes. An obvious example is the News Bulletins which may frequently contain scientific items that merit attention as news. Quite often, too, such items of scientific news are described in greater detail, are discussed and commented upon in the so-called magazine programmes such as 'At Home and Abroad', 'Matters of Moment', and 'Today' in sound radio, and the programmes 'Tonight' and 'Panorama' on television. Take the month of December 1959—here is a list of science items in the television programme 'Tonight'; this is one month and is by no means unrepresentative of what goes on month in month out:

1. Industrial Psychology Conference
2. Cactus
3. Skulls at the Natural History Museum
4. Forensic Medicine
5. The Sahara (on two occasions)
6. Woodpeckers
7. Prehistoric Mummies in Chile
8. Nuclear tests
9. Myxomatosis.

The variety of topics, the fact that these subjects are of news value at that moment, the attempt that is made to present the matter clearly, authoritatively and interestingly and that the programme 'Tonight' reaches an audience which is assessed at about eight millions, show the value of what may be called the indirect or incidental approach to science broadcasting. If it sometimes happens that the treatment of these items tickles the fancy of the uninitiated multitude rather than satisfying the specialist in his search for new knowledge, that does not rule them out provided they are treated seriously if not exhaustively, lightly perhaps but not trivially. There are other incidental ways of reaching large audiences; the inclusion of a scientist in a Brains Trust, an occasional reference to the application of a scientific principle in an 'Archers' episode, a documentary feature programme on a scientific topic or a play based upon the life of a scientist.

Then we come to programmes that are more direct attempts to convey scientific information. The oldest established of the science series is 'Science Survey' listening to which enables many science-minded laymen to keep up to date in a way which some at any rate would find difficult if they had to rely solely on scientific periodicals. 'Science Review' consists of comments by scientists on items selected from the scientific news of the month while the programme 'Who Knows' has a panel of scientists answering questions sent in by listeners.

With a wide appeal because they deal with out-of-doors matters which can be observed by anyone who has eyes to see and is interested to know what to look for are 'The Naturalist', 'The Naturalist's Notebook', 'Birds in Britain' and 'Country Questions'. These programmes, designed in varying degrees to interest as well as instruct, lay stress on the importance of close and accurate observation and convey something of the fascination and satisfaction that comes from an intelligent, informed appreciation of the familiar sights and sounds of the world around us.

An appreciation of the sights as well as the sounds is, of course, much intensified in television, and while it remains true, as I have already said, that there is a greater range and flexibility of appeal in sound broadcasting that is not to deny the quite considerable use that is made of television to inform, instruct and interest a wide public in scientific matters. Let us start with nature study where the use of films made by such experts in this field as Peter Scott, Hans Hass, and David Attenborough is an essential ingredient. A further example on which there is no need for me to elaborate because we have already heard an acknowledged expert on this subject, is the use of television cameras to reveal the wonder and fascination of the underwater world. Through the magic eye of the television camera and the film camera we can all be bird-watchers, deep-sea divers, jungle explorers, astronomers, investigators of wild life albeit at second-hand, but constantly prompted, directed and instructed by men or women whose inexhaustible patience, deftness and skill are so frequently placed at our disposal at the mere turning of a switch. If popular interest in the scientific presentation of wild life and plant life and in astronomy has grown in recent years, then it is undoubtedly true that broadcasting in sound and television has had a lot to do with it. It may be argued that arousing popular interest in itself does not get us very far, but enjoying vicariously a communicated experience may lead some by their own efforts to develop an absorbing interest which they may pursue for themselves, thus making their own lives and the lives of their acquaintances more satisfying and significant. May I say a further word about what is done in television. Schools television is now well established. There has been a term's programmes devoted to the presentation of mathematics; the series for sixth forms include one term's programmes in Science, and science subjects will form a large part of next year's increased provision of television broadcasts to schools. There will be series running the whole year on 'Discovering Science', 'Science and Life' and 'Natural History'. All this entails close consultation with education authorities, schools, and acknowledged experts, following procedures which have been established over the years by the Schools Broadcasting Council. An examination of science television programmes intended for adults also shows a tendency to plan series which follow a pattern and are intended to cover over a period of weeks a certain amount of ground.

'Science is News' seeks to give the facts behind the news of scientific advances and several different topics may be covered in the space of thirty minutes. It is placed early in the evening and is designed to interest the younger audience. This is a fortnightly programme which has been temporarily rested but which is to be resumed, I believe, in the autumn. 'Eye on Research' is another series of scientific programmes in which specialists working on research projects have an opportunity of explaining to the general public what they are doing and why and with what degree of success. The usual pattern of these programmes is to visit by means of television outside broadcasts cameras the laboratories where the research is conducted. The choice of subject is restricted to those which can be made intelligible and visually interesting to the main television audience within a period of half an hour and an overall balance is maintained between the main branches of science.

An example of a specially planned series devoted to a major scientific theme was the series of six thirty-minute programmes shown in the spring of 1958 under the title '500 Million Years' commemorating the centenary of Darwin and Wallace. These programmes presented the fruits of the most up-to-date thinking on the Theory of Evolution expounded by biologists of the highest academic standing in the country. In the spring of this year there was a series of six 'Eye on Research' programmes showing the current research projects of some of the Fellows of the Royal Society which were intended as a prelude to the Royal Society's tercentenary celebration, and there was a further series of five programmes under the general title

'Life Before Birth', a biological study of the beginnings of a human individual.

If the programmes I have mentioned constitute the staple fare of science broadcasting there are also the occasional, single, quite ambitious even spectacular programmes of an hour's length on a subject of current scientific importance. A major project of this kind in 1957 was 'The Restless Sphere', a programme to mark the beginning of the International Geophysical Year which was planned in consultation with the Royal Society and introduced by the Duke of Edinburgh. In 1958 there was a major programme describing the exploration of space by means of rockets and satellites. In 1959 there were two programmes of sixty minutes' duration dealing with the nature and behaviour of the living cell, and this year there will be at least one major programme with contributions from scientists inside and outside Great Britain to mark the tercentenary of the Royal Society. The general title 'Science International' given to these notable programmes conveys a suggestive overtone. And here I would like to mention the very good work done by some of our producers in Wales in collaboration with Dr. Glyn O. Phillips in presenting scientific programmes in the Welsh language. Dr. Phillips has a peculiarly sensitive nose for any scientist who speaks Welsh, and it is a very pleasant gesture on the part of Section X to invite him to address the Section in Welsh during this series of meetings.

May I go on to say something about the organisation of science broadcasts. The people who could speak with authority on the matter are the B.B.C's science producers in sound and television, some of whom are attending these meetings and have done so regularly for many years. They are specialist producers with scientific qualifications to support their skill and experience in the use of broadcasting. They are for the most part attached to the Talks Departments, sound or television, though some work mainly in Features or Outside Broadcasts Departments. They work in small units—the two most clearly defined perhaps are those found in Bristol and Birmingham. The Natural History Unit is based at Bristol, its location being a recognition of the distinctive work in this field done by the West Regional staff of the B.B.C. over many years, and a proof, if proof were needed, that the B.B.C. tries to guard against the dangers of over-centralisation which are inherent in any nation-wide authority, be it broadcasting or running the railways. In Birmingham, attached to the Midland Region of the B.B.C., there is the Agricultural Broadcasting Unit which also embraces gardening and horticulture, and is responsible for many series of programmes in both sound and television dealing with Applied Science in these matters. There is in London a small unit of science producers working in sound radio, there is a small unit of science producers in television, there is a Science Correspondent attached to the News Division, there are individual producers qualified in science working in the regions and there are science specialists working in the Schools Department. I suppose that there are in all some twenty-five B.B.C. producers whose particular and specific responsibility is the planning, editing and production of science broadcasts. That may seem to you to be too small a number to enable this major instrument of communication to play its proper part in this Age of Science. There are some both inside and outside the Corporation who take this view and there has recently been pressure put on the B.B.C. to increase the number of scientists on the staff and form them into a Science Department as there is a Talks Department and a Drama Department. This step is urged not only as a means of increasing the number and improving the quality of the science programmes but also as a way of recognising the status of science in the general programme output. Let's take first the question of increasing the number of scientists on the staff, i.e. men and women highly qualified, deeply interested, but dedicated essentially not to their particular branch of science but to the art of communicating, and they must be young, or at any rate not too old.

It often happens that brilliant young scientists are generally too immersed in their researches to be attracted to a whole-time job of mass communication. What time they can spare from research is devoted to teaching and this is often a condition of their research, but combining research with some teaching is a different thing from allowing oneself to be recruited to a full-time job of planning, arranging, editing and producing broadcasts by other scientists. This has a bearing on another circumstance which has its difficulties. Scientists have little need to explain what they are doing or thinking in such simple and concrete terms as to be

intelligible to the uninitiated or uninstructed, and a scientist who masters the art of communicating his thoughts to the general public, who goes down to the market-place as it were, who broadcasts or takes part in television programmes at all frequently runs the risk of being dismissed as a populariser or labelled glib and begins to lose his standing and authority as a scientist. Broadcasting may make reputations; it can also mar them. It is probably ill-advised to broadcast too frequently. What is reassuring is that more and more scientists are taking part in programmes. The work is well spread. I understand that last winter over a hundred different scientists were used in science programmes. In addition, twenty-five scientists broadcast in Welsh during the same period.

Let us consider this question of setting up a department of science broadcasting as there is of religious broadcasting (some people might well be happy to substitute the one for the other), or as there are Music and Drama departments. This is, of course, primarily a question of internal organisation for the B.B.C. but in recent months it has been a subject on which leading scientists representing august bodies, including this Association, have made their views known to the Corporation. To the proposal it can be objected that science is not one entity—there are so many sciences and so many branches of each science that a department would, if established, be constantly under pressure to increase its staff in order to represent this specialisation and that—an increase which would be out of all proportion to the output of science programmes that could be accommodated. Furthermore, to compare Science with Drama is to draw a false analogy: science exists in its own right and for its own sake—it is an increasing body of knowledge, it is a method of discovering new knowledge, it is a search for truth on many fronts, it is an attitude to life and to the world. Science is not something that comes to life only when it is communicated or performed as is Music or Drama. There are no public scientific observances as there are religious observances which can become shared experiences through being broadcast or televised. It is argued that the creation of a department of Science would confer status and prestige on science, and that may be, but it would not necessarily mean more science programmes or better science programmes. Indeed, it might well inhibit individual producers who are now freely encouraged to exercise initiative, consult outside experts, pursue their own plans, plough their own furrows. A department might tend to damp down the energetic pursuit of individual enthusiasms by dedicated producers. Yet again the B.B.C's departments for the most part are functional in terms of the main types of broadcasting, Talks, Outside Broadcasts, Drama, Features, Films, all concerned with special ways of communicating, not with the content of what is communicated. It is surely more important that there should be qualified scientists as Talks producers working in the Talks departments, as Outside Broadcasts producers, as film producers and so on, rather than that scientists should be segregated into a department of qualified specialists not in various forms of broadcasting but in various branches of science.

It may be that a department of science would make for easier and more effective consultation with outside bodies like the Royal Society, the British Association and various other scientific societies but there appears to be no lack of consultation and collaboration with such bodies now. While editorial responsibility for what is broadcast and televised must clearly remain the proper function of the B.B.C., the Corporation seeks and will continue to seek guidance and advice from those best able to give it. Should there be a B.B.C. Science Advisory Committee as there are Advisory Committees on Agriculture and Music? The membership of a committee to offer practical advice on scientific programmes would be difficult to make representative of the main branches of science and keep to a reasonable size. The absence so far of such a committee does not indicate any reluctance to seek and accept advice. It is really a question of whether a formal procedure conducted through an established channel is a useful and necessary support to the informal consultation of individual scientists that goes on all the time and the regular contact that exists between the B.B.C. and scientific bodies, such as the British Association.

I propose leaving the matter at that in order to go on to consider briefly to what extent the effectiveness of scientific programmes can be measured. There is no guarantee that what has been taught has in fact been learnt. The time and effort devoted to the planning, preparation and production of science programmes are not necessarily matched by a corresponding effort on the part of listeners and

viewers to enable them to profit appreciably from the programmes.

The task of assessing the effectiveness of a scientific programme is a difficult one. To begin with, its success must be related to the audience it is meant for. It is no condemnation of a Third Programme talk on, say, the limitations on the use of electronic computers in certain branches of medical research if it quite clearly failed to hold the attention, say, of a party of tourists listening to the car radio of the bus which is taking them to Blackpool to see the illuminations. It is not so much the talk meant for experts that is difficult to assess as the programmes meant for general consumption. The results of attempts from time to time to test the powers of comprehension of groups of people considered to be broadly representative of a general audience, by questioning them on the content of brief radio talks to which they had listened, do not justify extravagant hopes of the possibilities of radio and television for mass education. Even in the more restricted field of schools television the B.B.C's Head of Educational Broadcasting in a recent paper* on 'Television and the Teaching of Science' sounds a note of caution:

> We have hardly begun to find out in what circumstances and to what extent television can speed up the learning process, lead to longer retention of knowledge or act as a stimulus to further study.

The point has been made often enough that in the effort of comprehension much depends upon intelligibility, more on visual appeal, most depends on the interest already felt in the subject matter by the listener or viewer. Men interested in the Fens would look at or listen to, take in, understand, grasp and remember and be able to reproduce much more of a programme on Fenland than they would of a more simply worded, more agreeably entertaining programme, say on oil pollution of the sea coast.

What may seem to some of you a modest but none-the-less far-reaching assessment of the value of science broadcasting was suggested some years ago by Professor Andrade:

> If he (the listener) can at the end retain a general impression even if vague of the kind of thing that is going on at one point of the scientific front and of the kind of spirit that animates the man of science, then I think that the talk has been successful. In particular if the listener comes to think of science as something living and growing and not closed and finally certain; if he learns what kind of a 'why' the scientist is prepared to answer; if he notes the absence of arrogance that characterises people who know something but know that there is an infinity they do not know and contrasts it with the glib certainty and sweeping assertions of those who deal with political questions where all is uncertain—well, in that case science broadcasting is well worth while.†

The more immediate impact of particular programmes on individual listeners is measured by the B.B.C's Audience Research Department, which in addition to providing information on the probable size of audiences, has a highly organised arrangement through voluntarily recruited panels of listeners and viewers whereby the degree of appreciation can be indicated as a percentage.

This statistical expression of appreciation is supported by comments which help to give the B.B.C. the kind of information which a theatre manager or lecture agent would obtain from the box office returns or from the applause or catcalls of the audience and their remarks overheard on the way out. The Audience Research report on any one programme is itself a generalisation based on a sampling of the audience. To take a number of these reports and make a further generalisation about them gives us at best a general impression. Conclusion is too definite a word. What conclusion about a programme can one draw from these comments about the same programme:

> The conversation method . . . was ideal.
>
> The rather fussy interpolations of the (second speaker) seemed somewhat unnecessary.
>
> I am by no means convinced that the presence of a second speaker was necessary.
>
> The second person was an excellent interviewer and was himself very bright and on the mark.

except to say that one man's meat is another man's poison, or everyone to his taste, or as we say in Welsh, to his tooth. Here are some conflicting opinions of a television programme:

> I thought it was presented in a most attractive way and it was all very simply explained for ordinary folk like myself.

* *Contemporary Physics*, **1**, No. 3, p. 238, February 1960.

† *The B.B.C. Quarterly*, p. 161, January 1947.

It was moderately interesting but should it not have been shown on children's TV?

It was a case of taking a sledge hammer to crack a nut—an eminent man demonstrating elementary physics experiments.

Very sweet of this distinguished scientist to take the trouble to demonstrate so carefully what every school child learns at the age of ten.

The applause after the demonstrations reduced this to the level of a variety conjurer's act, with clapping after every trick. It lacked dignity.

Some fairly difficult physical concepts were made very interesting and very easy to understand by lucid explanations and excellently designed models and experiments.

Diverse and contradictory though they are these comments help the discerning producer and broadcaster to decide whether the intention they had in mind has been realised and whether the kind of audience they wished to reach has in fact been reached.

Perhaps I should leave the last word to a professional Television Critic who wrote in May of this year:

That such things as 'Eye on Research' really result in much increase in the diffusion of real knowledge academically measurable is unlikely. It is also unimportant. They must certainly help to create an atmosphere of respect for knowledge not as a pecuniarily rewarding possession but as something good in itself.

And if any of you are inclined to murmur, 'Well, that is something', I am tempted to conclude *That* is all.

BROADENING THE EDUCATION OF THE GRADUATE SCIENTIST

By **Sir PATRICK LINSTEAD, C.B.E., F.R.S.** *The Rector, Imperial College of Science and Technology*

About two years ago a special committee of the British Association instituted an enquiry on the broadening of the education of the graduate scientist and engineer, with the support of a generous grant from the Leverhulme Trust. I have directed this enquiry with the help of a committee of eight educationalists and Miss J. H. Lidderdale, who conducted the surveys and acted as secretary. The report, which was received this spring by the British Association, is about to be published by the Oxford University Press under the title of 'The Complete Scientist.'

The Leverhulme Study deals with the education of the growing number of scientists who, in their working lives, practise what they have learnt in principle at school and at university, i.e. professional scientists and professional engineers. It has been confined to broadening the education of the scientist and not to broadening in general since the very important professional element must be taken into account. The scientist or engineer is a man who may one day be responsible for crucial technical decisions. Therefore, any plans for broadening his education must be weighed and balanced against this compelling responsibility. It is hardly practicable, however, to put forward proposals for broadening without setting them against a background of what is going on in the specialist field so, although the content, standards or other aspects of science syllabuses or of specialist courses fell outside the terms of reference, we had to draw on our own experience and impressions to reach some general conclusions.

In proposing educational reforms, enthusiasm for the desirable may often overlook the financial possibilities. No systematic information existed to enable us to reach conclusions on extending the education of the intending scientist and technologist at the school stage, the first degree and postgraduate stages. We tried to keep our more exuberant thoughts in check by comparing them with such estimates of cost as we were to obtain.

Broadening the education of the graduate scientist is only one among a number of educational matters of current importance. Broadening the education of the non-scientist is just as important but it does not receive nearly as much attention. Several reasons for this can be discerned. Perhaps the main one is that the education of scientists is a matter of practical concern to a large number of people. More and more boys and girls are studying science—and, in the opinion of competent headmasters, more of the brighter ones are turning to science. If present trends continue, by 1965 10 per cent more of all G.C.E. 'A' level candidates will have two passes in science and mathematics. They form the reservoir which feeds the university schools of science and engineering. But today technical competence is not sufficient. Scientists and engineers are taking a growing part in the running of the world at what is called the policy-making and decision-taking level. Throughout their training they are exposed to the conflict between two contrasting pressures. In the first place they are drawn by a desire to explore fields of natural knowledge which are rapidly becoming more complex, and by the realisation that mastery of the scientific subjects opens the doors of a professional career. At the same time there is the gradual realisation that such knowledge is not enough, that there are truths and values which are not subject to scientific analysis. Most scientists

are conscious of this conflict and there is little complacency about it amongst those concerned with their education.

In the world today we need scientists and engineers who combine great technical competence with wise scientific leadership, vision and inventiveness. It was a tremendous benefit to us in our work that we were able to look at the years at school and university as a whole. This was an exceptional, if not unique, feature of our enquiry, most previous ones being confined to either the school or the university stage.

Such a wide province had, as a matter of practical policy, to be restricted in various ways. We confined ourselves to schools and universities in England and Wales; to the secondary stage, particularly the sixth form, in the schools; to those studying mathematics, physics, chemistry, general science, electrical and mechanical engineering at university at both the undergraduate and the postgraduate levels; to the present for our enquiries and to the decade 1965–75 when working out our suggested reforms in practical terms.

The report deals with that abstract entity, the average scientist. The outstanding man, whatever his subject, finds his own way of solving the conflict between the pressure of specialist subjects and other interests. I do not believe that a sharp distinction in intellectual habits can be drawn between scientists and non-scientists. It is true, however, that definite and different habits are developed by the two groups through the process of specialisation at school and university, but it does not follow that individuals are irrevocably fixed for life in one or other groove. Scientists are perhaps more rational, critical and analytical than the average, but the diversity of human nature and the breadth of human interests are sufficient guarantee to prevent them being classified as all of one type.

We approached our task by considering what provision ought to be made for non-scientific studies, and then considered what the consequences would be for the time spent on the specialist subjects. It was an enormous help that the Crowther Report became available at a useful stage in our deliberations. Its statistics were invaluable, although they were not sufficiently pointed for our purpose. We, therefore, embarked on two small surveys to obtain factual information from schools about curricula and time-tables of pupils trying for a university place. A larger-scale survey was required to discover what provision universities made for the study of non-scientific subjects or voluntary pursuit of interests in these subjects by their students of science and technology.

The reforms suggested are in essence a rephasing of the balance between scientific and non-scientific subjects throughout the whole period of education at school and university. In the earlier stages the school must take the initiative by providing non-scientific subjects in the formal curriculum. Below the sixth form the balance between scientific and non-scientific subjects is generally satisfactory, but much more time ought to be given to non-scientific subjects in the sixth form. At university the initiative for developing these interests should be with the student, but he will generally have to pursue them outside the first degree course which is already overloaded with specialised material. One aim should be to reduce the factual content of courses, another to deflect towards the study of general principles. Highly specialised subjects and more directly vocational studies should be left to postgraduate courses, many more of which should be provided. A four-year honours course for some students has been suggested.

But there are three problems of outstanding importance which must be solved if these reforms are to be put into practice, if breadth in scientific education is to become a reality. The stumbling-blocks all concern quality in education—university entrance, undue pressure on university places and quality in teaching at school—and they have been studied against the conditions likely to prevail in the next fifteen years.

The present pressure of numbers is so great that this is a peculiarly difficult time for qualitative changes. The main bulge in numbers will reach the universities in 1962 with a peak in 1967. A higher proportion of the sixth formers are qualifying to study science and technology. Pressure for places in science and engineering is already very great and, unless present policies are revised, it will get worse. Raising the total number of university places to 170,000 by 1970 will do little more than prevent a deterioration in the present position. The task of expansion before the universities in

the next fifteen years is so formidable that their room for educational manoeuvre and experiment will necessarily be restricted.

It is in the schools that the first steps towards a broader education will have to be taken. But it is the pressure on university places and the varied entry requirements which have led to the present unbalance in the sixth form curricula. More university places are required, and changes in the university entry requirements. A firmer basis for a broad education would be assured if the universities would accept not more than three 'A' level subjects in science and mathematics (with a narrower range of factual knowledge) and if they would also take into account attainment in non-scientific subjects.

BRITISH ASSOCIATION NEWS

Cardiff, 1960

Annual Meetings of the Association are—by intent—quite different from those of other professional and academic societies. Ever since its foundation in 1831 the Association has been 'on tour' in the belief that in this way a new set of people each year will be able to meet a substantial and distinguished group of practising scientists.

The organisation of a Meeting has become, therefore, very much of a combined operation in which—if one may put it this way—the Local Committee sets the stage and the Association provides the 'players'. While total attendance at an Annual Meeting is in no way a final criterion of the success of a Meeting the degree of participation by local people *is* felt to be so. The Association believes, in fact, that it fails in its purpose, if it does not encourage local people to feel that the Meeting is something which they can attend and profit from.

Even if judged in no other way but this the Cardiff Meeting (August 31 to September 7, 1960) must be counted an enormous success for there was a record student membership of 1700 (1400 from schools in Cardiff and its region) and a near-record total membership of 4122.

The local organisation of an Annual Meeting—to say nothing of the arrangement by the Association itself of the main scientific programme—is a formidable task and one which makes heavy calls on the time and energies of individuals already fully pre-occupied with their own jobs. The Cardiff Meeting was—by general agreement—both enjoyable and very much worthwhile: some sense of the Association's gratitude to its hosts in the city was expressed in the following resolution adopted by the General Committee on the last day of the Meeting:

At the close of this, the Association's third visit to Cardiff—and its first to the Capital City of Wales—the General Committee resolves to place on record its warm appreciation of all that has been done in the City, the University College of South Wales and Monmouthshire, and, indeed, in the Principality to ensure the success of the Association's 122nd Annual Meeting. Everything has been done which could contribute in any way to the effectiveness and success of the Meeting and the General Committee expresses sincere thanks on behalf of all members of the Association:

To the Rt. Hon. the Lord Mayor of Cardiff, the City Council and their Officers and staffs.

To the President and Council of the University College of South Wales and Monmouthshire.

To the Chairman, Officers and members of Local Committees whose thorough preparations have contributed in so great a measure to the success of the Meeting.

To the President and Council of the National Museum of Wales.

To the Principal of the Welsh College of Advanced Technology and his staff.

To all firms, organisations and institutions in the region who have so freely opened their premises to members of the Association and to the very many individuals who have entertained members so generously in their homes.

To the educational authorities, teachers and students of Cardiff and its region for their enthusiastic support.

To the Chief Constable and other officers of the police of Cardiff for their efficiency and courtesy.

To all the voluntary helpers, not forgetting the Boy Scouts, who have given so freely of their time and energy in service of the Association during the week of the meeting.

To the citizens of Cardiff and all others whose unfailing kindness and interest have resulted in an outstandingly happy and rewarding meeting and in a membership of over 4,100—a number which has rarely been equalled.

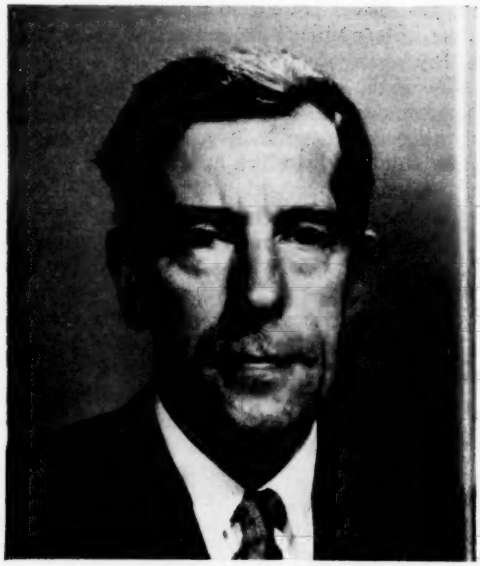
An Historic Meeting

If the Annual Meeting was successful, it was also—in a number of ways—an historic one. Historic because, for the first time in the Association's history, the whole of one day was devoted to a plenary session on World Food and Population. In practice this meant that the fifteen separate Sections of the Association gave up their own meetings and papers on that day and met as a corporate group to discuss a problem of overriding significance and of general scientific interest.

The symposium, which was attended by some 2000 members and has since been widely reported in the national press, was planned partly as a 'curtain raiser' to F.A.O's five-year 'Freedom from Hunger Campaign' launched in July of this year. One thing was clear from the debate—'the world can feed its multiplying population by obvious methods which science and commonsense can determine. It is not the numbers which are frightening: it is the rate of increase.' The moral aspect of the problem was emphasised by the Bishop of Llandaff in a sermon given in Llandaff Cathedral on September 4 in which he insisted that 'there is no moral justification for maintaining the standard of living which the West has become accustomed to while two-thirds of the world are below subsistence level'.*

The symposium, however, is only the first of the many contributions the Association hopes to make to public discussion of the vital issues involved. The Council has already set up a powerful standing Committee to undertake continuing studies of the scientific aspects, to arrange for publication of the material arising out of the general and sectional discussions at Cardiff, and to co-operate in the provision of a series of lectures on the subject to interested audiences throughout the country. The Association has also asked the Government for representation on the National Campaign Committee to be set up by the Minister of Agriculture, Fisheries and Food in support of the 'Freedom from Hunger Campaign'.

* The full text of the sermon has now been published and may be obtained at a price of 1s. from Miss Horley, Llys Esgob, Llandaff, Cardiff. Any profit arising from the sale of copies will go to the Oxford Committee for Famine Relief.



G. F. MITCHELL

(see p. 313)

New Statutes and Installation of the President for 1960–1961.

The Meeting at Cardiff was noteworthy for a number of other 'firsts'. It was announced that H.M. The Queen had been graciously pleased to approve certain changes in the Association's Charter and Statutes. These add to the main objects of the British Association 'to maintain, develop and extend the social and international relations of science', while maintaining the rule that the Association does not contemplate invasion of the ground occupied by other Institutions. They also give the Association the power 'to establish and set up Branches of the Association' and to set up Committees, 'members of which need not be

members of the Association'. Adequate provision is therefore made for the organisation of the important inter-Annual Meeting work begun two years ago, with the help of special donations from industry.

A further consequence of the new Statutes was the installation of the new President, Sir Wilfrid Le Gros Clark, F.R.S., at a general assembly of members held on the last day of the Meeting at Cardiff. A profile of Sir Wilfrid, an outstanding anatomist and student of the ancestry of man, appeared in the September number of this journal.

Members who were unable to attend the General Assembly may be interested in the following summary of accomplishments during the year—all of which were achieved after due consultation with interested educational bodies and other institutions:

(i) The all-the-year-round activities of the Association have taken two main forms—the creation of a Central Lecture Service and the establishment of Area Committees. Between January 1, 1959, and July 1, 1960, audiences at scientific lectures arranged by the Lecture Service and the Area Committees numbered 102,000, of whom 65,300 were young people.

There are now twenty Area Committees established or in the course of formation. They cover almost the whole of Britain and when they are all working fully they are expected to provide lecture audiences of over 250,000 annually. The Area Committees are widely representative of scientific and educational interests in their localities and are encouraged to experiment in the presentation of science.

(ii) A large part of the activities of the Central Lecture Service and of the Area Committees is concerned with the Association's Young People's Programme, as part of which three full-scale Junior British Association Meetings have been held at Nottingham, Birmingham and Belfast, together with other two-day young people's programmes each catering for some 2000 students.

(iii) Much work has been done to encourage the production and use of suitable visual aids and the building up in university departments and elsewhere of stores of demonstration material for lectures to lay and school audiences. Receptions have been arranged by large industrial firms to show science teachers and educational authorities what material is available and to invite criticisms of it. A full-scale survey of the use of visual aids for science in secondary schools has been completed and a report has appeared in the September number of *The Advancement of Science*.

PROFESSOR E. ESTYN EVANS

(see p. 293)

(iv) The Association's publications have been restyled and *The Advancement of Science* now appears six times a year. The work of three British Association Study Groups has been completed and the following reports published: 'Science in Industry —a Policy for Progress'; 'Decimal Coinage and the Metric System; 'The Complete Scientist', the report of the Leverhulme Study Group, is due to appear during the autumn. The Association has also published a *Guide to Press Arrangements for Scientific Conferences*, prepared by the Association of British Science Writers.

(v) The Association has co-operated in a second successful London International Youth Science Fortnight which was opened in July, 1960, by H.R.H. The Duke of Edinburgh.

(vi) A second annual series of lectures on 'Communication in the Modern World' organised by the Association in conjunction with Granada TV Network Limited took place during September and October. It is expected that the texts of the lectures given by Professor G. Beadle, Professor H. J. Eysenck, and Lord Adrian will be published in *The Advancement of Science*.

A full account of the Association's work during the year and a record of the scientific programme of the Annual Meeting at Cardiff will be published and distributed to members, associate members, life members and corporate members, towards the end of the year. Copies of this annual report may be purchased by non-members of the Association for 2*s*. 6*d*. A selection of the many papers given during the Annual Meeting at Cardiff will appear in successive issues of the journal.

A. B. OLDFIELD-DAVIES, C.B.E., M.A.

(see p. 327)

STELLAR MOTIONS*

By Dr. R. van der RIET WOOLLEY, O.B.E., F.R.S. *The Astronomer Royal*

Although the stars proper as distinct from the members of the solar system, the planets and comets, have approximately fixed positions relative to one another, as seen in the sky, there are in fact very small movements in the apparent positions of the stars relative to each other. They require very painstaking observation in order to ascertain their amounts with any precision, or even to set up effective arguments based on the statistics of these motions in the many cases where they are so small that we have no reliable measures of them. This is because the stars are so far off that although the motions are in fact quite rapid, the apparent motion from year to year is quite tiny.

I shall have to introduce at once the parsec as the unit of distance. The fundamental method of determining the distance of stars is of course by triangulation, using the diameter of the Earth's orbit round the Sun as a base: and the angle subtended by the star at opposite ends of this base and divided by two is called the parallax of the star. A star is said to be at a distance of one parsec if this parallactic angle is exactly one second of arc, and the parsec, which is almost exactly $3 \cdot 10^{18}$ cm., is the unit of distance employed in stellar astronomy.

Now it can be seen by elementary arithmetic that a velocity of one kilometre per second is equal to a velocity of one parsec in 10^6 years, almost exactly. We can make use of this figure to calculate the sort of movements that we should expect to see with our instruments which would correspond to a stellar velocity of one kilometre per second. Consider a fairly close star, namely one 20 parsecs distant from the Sun, moving with a velocity of one kilometre per second normal to the line of sight. In one year it moves one part in twenty million of the distance, and since there are 206,265 seconds of arc in a radian, such a star moves apparently almost exactly one second of arc in a century. These movements are observed in two ways. In the first place direct observations of star positions can be made with meridian instruments, in particular transit circles. With such an instrument a catalogue of star positions can be drawn up, and comparison of star catalogues made at different epochs, differing say by thirty years or more, will give proper motions of the stars observed. But there are difficulties. In the first place we must ask relative to what co-ordinates are the star positions observed. The actual choice made by astronomers is peculiar, since the co-ordinate system employed is a moving one. This arises from a practical necessity, which can best perhaps be realised by comparison with the co-ordinate system employed in locating places geographically on the Earth's surface. Here we define latitude with reference to North and South poles defined by the Earth's rotation, and longitude with reference to a conventionally adopted point on the Earth's surface, to wit Greenwich. Now it is in fact a difficulty that the poles of the Earth's rotation do wander slightly relative to the Earth's crust, but a mean pole fixed relative to the Earth's crust can be adopted, and a co-ordinate system fixed relative to the crust can be set up. So far so good. But when we turn to the stars it is not practicable to set up fixed points on the celestial sphere. Each observatory may form its own estimate of the direction among the stars of the pole of the Earth's rotation, and this is done perpetually at observatories which are engaged on making fundamental catalogues of star positions. It is now necessary to choose a suitable definition of the zero point of the co-ordinate which corre-

* Address delivered to Section A (Mathematics and Physics) on September 2, 1960, at the Cardiff meeting of the British Association.

sponds to geographic longitude, namely the co-ordinate which astronomers call Right Ascension.

Suppose one were to choose a particular star and adopt it by convention as the zero of Right Ascension: Sirius, for example, is chosen to be the Greenwich of the stars. Then all observations of Right Ascension must somehow be referred to this particular star, and we have no means of knowing whether our zero is moving or not (or, more accurately, of knowing how much it is moving, as there is no justification for supposing that the movement relative to the Sun of any star is zero). Instead, meridian observers choose a criterion which can be realised comparatively

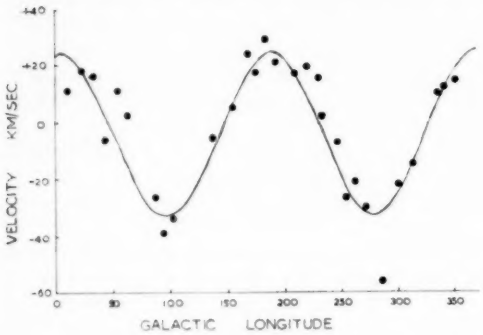


Fig. 1.

simply, namely the position of the Sun when it is 90 degrees from the celestial pole. This notion has to be refined on account of irregularities in our motion, and is in fact the intersection of the equator and the ecliptic, which is a moving point relative to the stars on account of precession and nutation, the regular movements of the plane of the Earth's orbit which take place on account of the attraction of the Moon and planets. As far as stellar motions are concerned, we have to take account of the fact that these two zeros of our co-ordinate system, pole and equinox have to be realised as accurately as possible by our instruments, and it is probably fair to say that the best contemporary work will realise these points with an accuracy of about three-hundredths of a second of arc.

Each meridian instrument produces observations which are affected not only by random errors but also by systematic error, as for example error due to the flexure of the instrument which may amount to six-tenths of a second of arc. If one determined stellar motion solely by comparison of positions observed at different epochs by the same instrument, as for example Airy's transit in A.D. 1930 minus Airy's transit A.D. 1900, some of these systematic errors might be eliminated from stellar motions: but in practice it is necessary to compile catalogues of star positions by combining the observations made with many instruments, and then differences between the systematic errors of the various instruments must enter into the results, though the compilers of catalogues attempt to ascertain the systematic errors of individual instruments and compensate for them.

It may be said that where a star has been frequently observed with the most reliable instruments, it is possible to state its proper motion with an accuracy of five-thousandths of a second of arc per annum: but it must be said at once that comparatively few stars have been observed frequently enough to allow of this accuracy.

We turn now to the determination of relative stellar motion by photography. If a photographic telescope has a focal length of 3 metres, a second of arc in the sky corresponds to a displacement of 15 microns in the focal plane. Hence our standard star distant 20 parsecs and moving with a transverse velocity of one kilometre per second moves $1\frac{1}{2}$ microns relative to 'fixed' stars every ten years, and comparison of new and old photographic plates affords a method of determining stellar proper motions. These can be linked to absolute proper motions determined by meridian circles in each case where the pair of plates contains a sufficient number of reference stars. Otherwise the motions determined photographically are relative ones—the movement, for example, of a close star moving fast relative to a background of very slowly moving stars.

Since each telescope produces images many microns in diameter and possibly displaced in various parts of the field relative to the positions in which they would be placed by an ideal instrument, it is best to work by repeating old plates taken by an instrument still existing. A very valuable series of plates was commenced in the 'eighties of the last century at a number of observatories, it being intended to photograph within one or two decades the entire sky. This

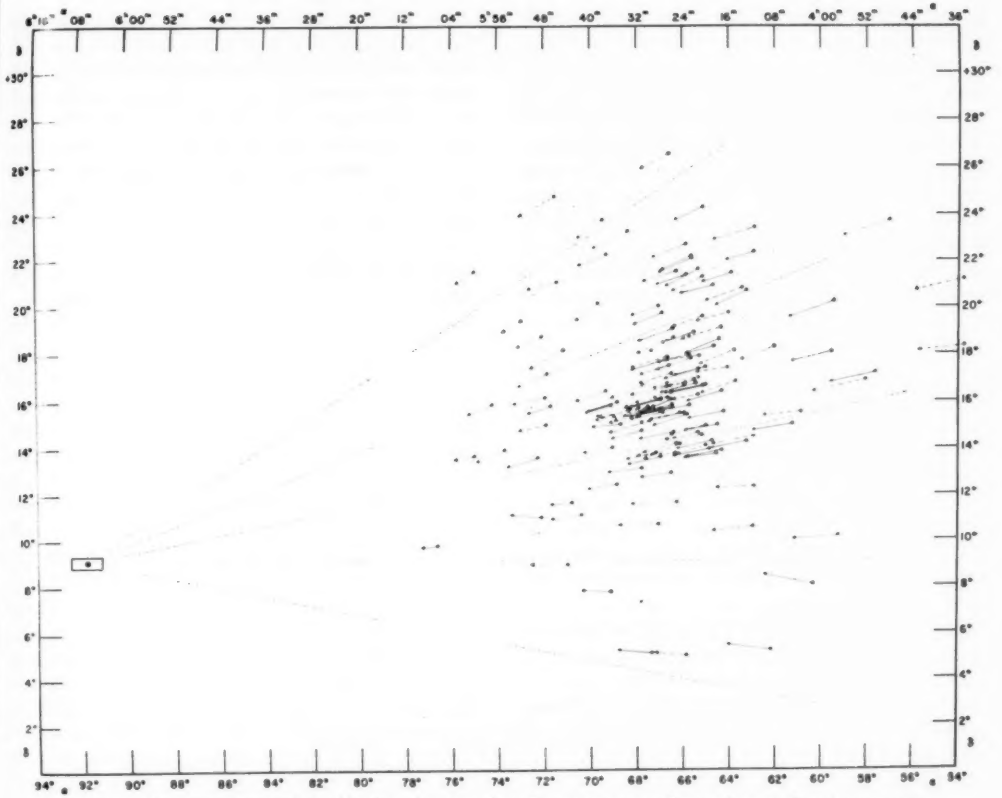


Fig. 2.—Proper motions of the Hyades, showing that the motions are directed towards a convergent.

campaign produced the so-called Carte du Ciel and the Astrographic catalogue. The sky was divided into various zones and the work allotted to various observatories which acquired virtually identical instruments, the so-called Astrographic refractors. The work proceeded at divers rates in divers places, but in some zones at least astrographic plates were secured and preserved. Some zones have even been repeated. Where old astrographic plates are available, dating from the last century, excellent relative proper motions can be secured by taking repeat plates now.

Apart from this, it is a matter of difficulty to secure old plates taken with any instrument in sufficient number to cover any extended area of the sky in which one may be interested.

We now come to motion in the line of sight: motion, of course, normal to the transverse motion that we have been discussing and which is revealed by comparison of new and old star positions. Motion in the line of sight or radial velocity is revealed by the Doppler Effect, and once again measurement of a velocity of one kilometre per second is a matter of measuring microns. To see this we must remember that the Doppler shift divided by the wavelength of the light used is equal to the velocity of the star divided by the velocity of light, or $\Delta\lambda/\lambda = v/c$. Since 1 km./sec. is one part in 300,000 of the velocity of light, the Doppler shift corresponding to 1 km./sec. is four-thirds of one hundredth of an angstrom unit at a wavelength of 4000 Å. Using the rather high dispersion of 10 Å/mm., the shift to be measured works out at $\frac{4}{3}$ of a micron. In principle the method

is very straightforward. A stellar spectrum is photographed, and on either side of it is placed the emission spectrum of a terrestrial source such as the iron arc produced at the same time by the same spectrograph, and in principle one simply measures the displacement between the stellar lines and the terrestrial lines. A correction has to be made for the velocity of the Earth in its orbit round the Sun, but although this may amount to 30 km./sec. its amount is always known with an accuracy greater than $\frac{1}{10}$ km./sec. Similarly, a smaller correction equally well known must be applied on account of the Earth's rotation. Nevertheless difficulties arise. These are that in some stars, the so-called early type stars, the stellar lines are diffuse and cannot be measured accurately. Again, each line in the terrestrial source and in the stellar spectrum is not necessarily pure but is or may be a blend of various lines, not separated by the spectrograph employed. This is clearly an effect which is less troublesome the greater the dispersion that is employed. Lastly, errors may arise due to want of perfect collimation of the parts of the spectrograph and to want of perfect placing of the star image on the slit of the spectrograph. This last error is called guiding error and is most troublesome in modern fast spectrographs which are specially designed with long-focus collimators in order to open out the slits and economise starlight.

There is of course not much light from a star, and long exposures are required to produce high dispersion spectra of all but the brightest stars, even if a telescope of great aperture is used to collect the starlight in the first instance. Now from the point of view of resolution of lines on the final photographic plate, it is clear that if a given photographic emulsion will resolve no better than say 25 microns, no extra resolution is gained by closing the slit of the spectrograph more than this: or rather, more than the optical equivalent of this, if the collimator and camera are of different focal lengths. Also, the image of the slit in the focal plane is diminished in the ratio of these focal lengths. Hence by employing a long-focus collimator and a short-focus camera we arrive at a wide slit, one that accepts most of the starlight offered to it by the telescope, while securing at the same time the maximum resolution of which the photographic plate is capable.

Yet it is the function of the slit of a spectrograph to exclude light. It has no other function. And we arrive at the position that a fast spectrograph which has a wide slit requires very accurate guiding of the stellar image in the jaws of the slit, if the centre of gravity of the starlight is not to be nearer one jaw or the other and produce a small shift in the stellar lines in the focal plane resulting in an error in the stellar velocity.

In practice, it is exceedingly difficult to determine stellar radial velocities with an error of less than one kilometre per second. In early type stars, and in the case of faint stars which can only be tackled with low dispersion spectrographs, the error to be expected is several kilometres per second. While the degree of precision depends on the brightness of the star in so far as high dispersion spectrographs cannot be used on faint stars, the determination of radial velocity is not directly dependent on the distance as is the case of transverse motions. In the one case, if the star is bright enough we can observe its velocity no matter how far off it is, and in the other we can observe its motion if it is close enough, no matter how faint it is.

Notice that the transverse motion, the proper motion as it is called, is only an angular motion and cannot be converted into a linear motion in kilometres per second unless we know the star's distance. This latter requirement is not easily met with any precision. As far as the direct measurement of stellar parallax is concerned the standard error is usually about $0''{\cdot}008$, so that only in the case of the closest stars with parallaxes exceeding $0''{\cdot}160$ is the distance known to better than five per cent. Other arguments, however, exist in special cases. One such case is that of the moving cluster, and we turn to consider the moving cluster in Taurus.

The moving cluster in Taurus, or Hyades cluster, is a large group of stars occupying an area of about 15 degrees square in the sky, whose proper motions converge to a point some 30 degrees away from the centre of the cluster. As the Hyades are seen from here, from the neighbourhood of the Sun, there are some stars between us and the cluster and many more on the other side of the cluster. For example the very bright star Aldebaran appears in the middle of the cluster but is not a member of it, and the majority of stars in the

direction of the Hyades are not members of the cluster at all. It becomes a matter of sorting out the motions to determine which stars are and which are not members of the Taurus or any other open star cluster. (Fig. 2)

From the convergence of the directions of the proper motions of these stars it was concluded that the true motions of the stars must be equal and parallel with a great degree of accuracy. The point is discussed for example in Eddington's *Stellar Motions and the Structure of the Universe* which was published in 1914. Now if it be supposed that the stars are 10^{10} years old, and that they have always formed a cluster with a diameter not greater than the present diameter, namely about 10 parsecs, it must be supposed that the velocities do not differ by more than 10^{-9} parsecs per year, or one metre per second. We no longer suppose that these stars are as old as 10^{10} years, and we no longer consider that mere parallelism of motion is enough to keep stars together if they are not bound by mutual gravitation attraction. This is because we now recognise that the stars are moving in the gravitational field of the galaxy, and the condition of strictly parallel motion must be replaced by the condition that the stars' galactic orbits must all have the same period. But this latter condition approaches the condition of parallel motion if the stars occupy a volume of space small in comparison with the volume of the galaxy, and we may suppose that the velocities of the Hyades stars are equal and parallel within one per cent.

If this be admitted we have at once a powerful and accurate method of determining the distances of the Hyades stars from their proper motions and radial velocities.

Fig. 3 shows a diagram of the observer at O and the star at S moving in the direction OR. This direction is the radiant of the cluster and it may be determined from the convergence of the proper motions. Now let the angular distance of the star S from the radiant Q be denoted by λ; it can of course be measured. Then if the velocity of the star be V the radial velocity is $V \cos \lambda$ and the motion perpendicular to the line of sight is $V \sin \lambda$. The angular proper motion is the transverse motion $V \sin \lambda$ divided by the distance d, and it is clear that the radial velocity gives the velocity V and that the observed proper motion determines the distance of the star.

If the stars are indeed members of a moving cluster the observations of their separate radial velocities will give consistent values of the cluster velocity V within the limits of experimental error, or rather, if these observations are precise, they will determine an upper limit to the dispersion round the cluster velocity V which the stars may possess.

For example, in the Hyades the cluster velocity is observed to be 44 km./sec., and the bright star γ Tauri is observed to be 31° from the radiant. This means that the transverse velocity is 44 times

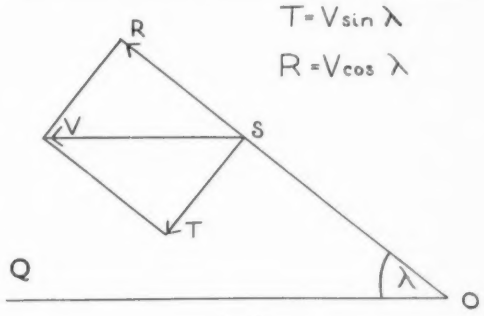


Fig. 3.

sine 31° or 22·7 km./sec. However, the observed proper motion is 0·122 sec. of arc per annum, or one part in 1,690,000 per annum. Remembering that 1 km./sec. = 1 parsec in 10^6 years, we deduce that the distance of the star is 37·9 parsecs. The parallax is the reciprocal of the distance in parsecs, or $\pi = 0''{\cdot}0264$. The accuracy of this distance (and this parallax) is probably of order one or two per cent, and the distance is much more accurately known than it would be from the ordinary method of trigonometrical parallax which happens to give for this star $\pi = 0''{\cdot}023$.

The proper motions and radial velocities in the Hyades are now being examined at the Royal Greenwich Observatory in the light of new material. New proper motions have been found from Greenwich meridian circle observations made in the early 1930's. It is found that the directions of the proper motions intersect a small area which is consistent with the errors in these observed directions being $\pm 1°$ in the best observed cases. This standard error may arise from errors in the observations themselves or it may arise from an

actual dispersion of the stellar velocities from the mean velocity of the cluster. Since one degree is one part of fifty-seven of a radian, a dispersion of a degree in the direction of a transverse velocity of 23 km./sec. corresponds to a dispersion in velocity of $\pm 0{\cdot}4$ km./sec. Not all of this can be attributed to the star velocities, and indeed the reduction of the observations suggests that most of this dispersion is due to observational error. The stars are being observed again in the current meridian programme at Herstmonceux. As the time base from the first available accurate observations is now much increased, the new proper motions may be expected to be very accurate and may reveal the true velocity dispersion of the cluster. At all events they should set a new upper limit to it.

The radial velocities have been examined through the courtesy and co-operation of Mount Wilson Observatory, which sent a number of existing spectra of the red giant stars in the Hyades for measurement. Further, the Mount Wilson Observatory allotted me, through its Guest Observer programme, a number of nights on the 100-in. telescope. A large number of spectra of Hyades dwarfs were secured and these have been measured for radial velocity at Herstmonceux. The measured velocity dispersion is five-tenths of one kilometre per second, but most of this must be attributed to observational error and the true dispersion of the Hyades star motions is probably three-tenths of one kilometre per second or even less.

The accurate distances determined from motions may be used to construct absolute magnitudes of the stars in the cluster. It is of course a difficulty in attempting to account for the physics of stars in general that the brightness as we see it or apparent magnitude has no particular significance unless the distance of the star is known so that a correction based on the inverse square law can be applied and the brightness of the star at a standard distance of 10 parsecs, or absolute magnitude, can be found. Clearly in the case of the moving clusters, where the distances may be found accurately, good values of the absolute magnitudes of the stars can be constructed if accurate observations are made of the apparent magnitudes. If these observations are made in two different colours, it is possible to plot the famous Hertzsprung Russell diagram, or as it is now universally called, the HR diagram, which is a plot of colour as abscissa and absolute magnitude as ordinate.

Fig. 4 shows the diagram as it was drawn

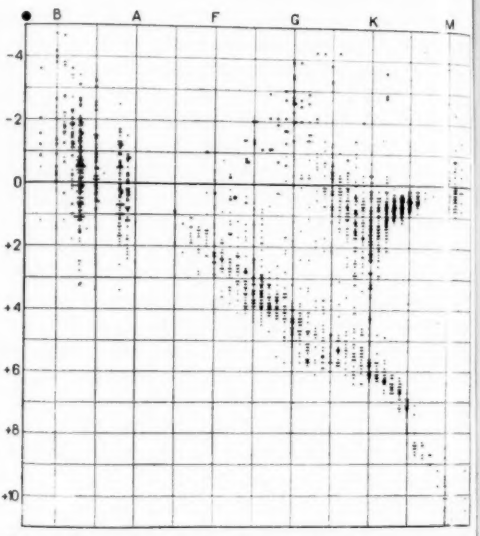


Fig. 4.—The statistics of stellar magnitudes and spectral types as originally drawn up (taken from Eddington's *Internal Constitution of the Stars* 1926).

up independently and practically simultaneously by Hertzsprung working in Europe and by Russell working in America. The stars are roughly separated into giants and dwarfs, this separation being marked at the right-hand side of the diagram on which the very red stars lie, while the two branches of the diagram meet on the left-hand side which is occupied by the bluest stars. This diagram, the original Hertzsprung Russell diagram, is a blurred one, partly because each point is subject to considerable uncertainty, especially in the up and down direction, on account of the fact that the stellar parallaxes used in constructing absolute magnitudes are for the most part very imperfectly known: and also because all stars are taken together in this diagram if they are accessible to observation, so that the diagram may and in fact does lump together and confuse many distinct classes of stars. For example, consider Fig. 5 which shows the HR diagram of the

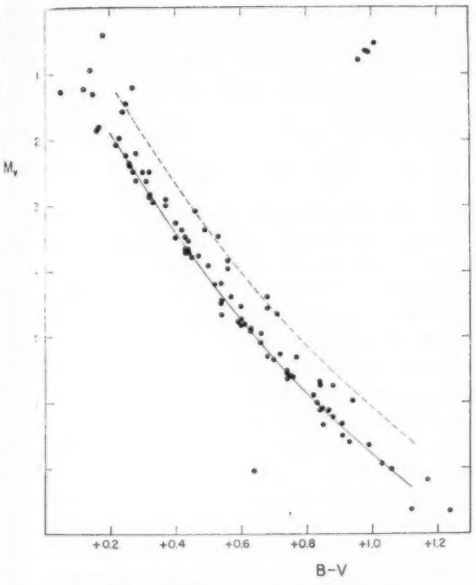


Fig. 5.—HR diagram of the Hyades according to Eggen.

Hyades alone. This particular diagram was constructed by Dr. Eggen from accurate photoelectric observations of the brightness of Hyades stars in two colours. A very distinct main sequence of stars, running from red dwarfs at the bottom right-hand side of the diagram diagonal up and to the left may be seen. One may see also in the Hyades four red giants in an isolated position on the diagram. Eggen published rather similar diagrams for other nearby clusters, including the Pleiades, and showed that the main sequences were practically identical. This opens up a method of determining the distance of any object whose HR diagram shows a distinct main sequence, as the distance of the object, if unknown, can be adjusted and the HR diagram slid vertically up and down until the main sequence lies over the main sequence established from nearby clusters. There is a danger in this procedure arising from the presence of absorbing matter in interstellar space, about which we will say no more than that it can be circumvented by appealing to observations in the near ultra violet.

Not all clusters show an HR diagram like that of the Hyades.

Fig. 6 shows a composite diagram drawn up by Dr. Sandage to represent the HR diagrams of a number of objects. In some of these the very bright stars, those at the top of the diagram, are red—they lie over towards the top right-hand side of the diagram. In other words the brightest stars are red giants. Very far reaching conclusions about stellar evolution are drawn from these diagrams, but we will go no further into the matter on this occasion but will return to the topic of stellar motions.

It is now a commonplace that all the stars that we can see with the naked eye and also hundreds of thousands of stars too faint to be seen or even photographed without comparatively large telescopes are grouped into a configuration something like that of the great nebula in Andromeda. The spiral appearance of this nebula suggests that it is rotating, and we may ask whether our own system of stars is also rotating. Our own system is called simply the galaxy. If the galaxy rotated as a wheel rotates, all at the same angular rate, we should not be able to observe this rotation by examining the motions of the stars which compose it: but if it rotated as a whirlpool rotates, the inner parts moving faster than the outer, we could in principle observe a differential motion between inner and

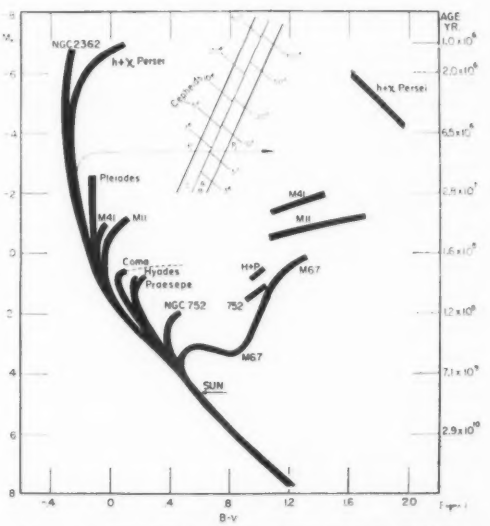


Fig. 6.—Composite HR diagram according to Sandage.

outer rings of stars. This effect has been looked for and found in the statistics of stellar motion. If the effect is greater, the greater the distance from the Sun, it should be most easily seen in the statistics of the radial velocities of the most distant stars, rather than in proper motions. This is because for a given velocity difference proper motions decrease, with the distance of the star, whereas radial velocities do not, since the Doppler shift is independent of the distance of the star.

The first convincing observations of differential rotation were made in Canada in the early 1920s, with the 72-in. telescope at Victoria, in British Columbia, under the direction of J. S. Plaskett. Naturally in Canada one is only able to observe the Northern sky, and the work has been completed quite recently in Pretoria, in South Africa, with the 74-in. telescope at the Radcliffe Observatory, under the direction of A. D. Thackeray. It can now be said that a ring of stars 1000 parsecs further out from the centre of the galaxy than the Sun's position is falling behind the stars in the Sun's neighbourhood at the rate of about 30 km./sec., and, conversely, that a ring one kiloparsec further in towards the centre of the galaxy is gaining on us at the same speed of about 30 km./sec. These observations alone do not establish the actual rate at which our own ring of stars is going round the centre of the galaxy, V, nor the distance to that centre, R, or even the ratio V/R. But a further appeal to stellar motions does yield the ratio V/R, in this way.

Each star as it proceeds round the centre of the galaxy moves on an orbit governed by the attraction of the galaxy as a whole. As the attracting mass is not concentrated at a point, nor distributed with spherical symmetry so that it acts as if it were concentrated at a point, the orbit of each star is not exactly an ellipse. But if a star's motion is not far from circular, it may be regarded as motion on a small elliptical epicycle borne by a centre or deferent which itself does move with the circular velocity. Each of these epicyclic ellipses has major and minor axes whose squares stand in the ratio of V/R to $\frac{1}{2}(V/R+dV/dR)$. Now we cannot actually observe these ellipses, as they take many millions of years to describe, but we can observe the velocities with which the stars trace them out; and it can be shown that the average (root mean square) velocity of a star in the direction in and out from the galactic centre has the same ratio to the average r.m.s. velocity perpendicular to the galactic centre (differenced from the circular velocity) as the ratio of the axes of the actual ellipses.

In the case of stars within 20 parsecs from the Sun the trigonometrical parallaxes are reasonably reliable and the distances of the stars are known with tolerable accuracy. It is therefore possible to convert the proper motions in angular measure per century to transverse velocities in kilometres per second. These can be combined with radial velocities measured directly in kilometres per second to compute all three components of the motion, in kilometres per second, in any desired system of rectangular co-ordinates.

It is convenient to choose rectangular axes such that the x axis points away from the centre of the galaxy: the y axis lies in the plane of the galaxy, and is in the direction in which the galactic rotation carries the Sun; while the z axis is perpendicular to the galactic plane. The velocities along these three axes are called u, v and w.

The first thing to do is to calculate each star's velocity components u, v, w in this system, from the observed parallax, proper motion and radial velocity. The best list to use is one compiled by Gliese, who gives the particulars of 742 such stars. If we add the Sun itself there are 743.

First of all we may take the mean, which turns out to be

$$u_0 = +11 \text{ km./sec.}$$
$$v_0 = -17 \text{ km./sec.}$$
$$w_0 = -7 \text{ km./sec.}$$

This is the average of the stellar motions relative to the Sun: so that the Sun itself is moving in the reverse of this motion, relative to the centre of the local star system. This is called the solar motion, and it is $+21{\cdot}4$ km./sec. in the direction R.A. $18^{h}\ 13^{m}{\cdot}2$, Dec. $+30^{\circ}{\cdot}5$, that is to say, a point in the constellation Hercules.

For each star we can subtract the mean motions u_0, v_0, w_0 from the particular motion of the star u, v, w and find $(u-u_0)$, $(v-v_0)$, $(w-w_0)$ the three components of the star's motion relative to the mean of the rest. It is found that in each co-ordinate it is possible to represent the distribution of the

motions, with some degree of fidelity, by a Gaussian distribution. For example, if

$$n(u-u_0)\Delta(u-u_0)$$

is the number of stars with $(u-u_0)$ components between $(u-u_0)$ and $u-u_0+\Delta(u-u_0)$, then

$$n(u-u_0)=\frac{nj}{\sqrt{\pi}}e^{-j^2(u-u_0)^2}$$

with similar expressions for v and w, but with different values of the coefficient j. This is known as an ellipsoidal distribution of velocities.

It fails in particular to represent at all well the larger velocities in the y direction, as these are always asymmetrical, and are deficient in high positive v's. This has a clear explanation, as a star with a fairly high positive v would have the velocity of escape from the galaxy; hence high positive v's are absent.

It is convenient sometimes to work with the dispersions σ_u, σ_v, σ_w of the velocities: these are such that $j\sigma=1/\sqrt{2}$, in the case of a Gaussian distribution. For late type stars, σ_u is about 39 km./sec. and σ_v 23 km./sec. The dispersion perpendicular to the galactic plane is smaller, being about $\sigma_w=18$ km./sec.

Some deductions may be made from these figures immediately. The first is that the stars have not reduced their velocities to a steady state as a consequence of collisions. One does not of course mean head-on collisions: but two stars which pass close enough to each other to be influenced by their mutual attraction have to describe hyperbolic orbits round a common centre of gravity. Relative to this centre of gravity there is no exchange of energy, and the stars move away from each other, in the end, with the same velocity that they approached: but if the centre of gravity of the star pair is itself moving relative to the galaxy there is an exchange of energy. After enough collisions, equipartition of energy will set it. The fact that σ_u is much larger than σ_w shows that we are nowhere near it yet.

However, the relation between σ_u and σ_v fits in very well with other ideas. Each star is moving under the central attraction of the galaxy as a whole. As the attraction is not that of a point source, the orbits are not exactly closed ellipses, but they may be regarded as elliptic epicycles centred on a point moving round the galaxy with the circular velocity. The ratio of the axes of these ellipses is reflected in σ_u and σ_v, and tells us something about the departure of the galactic attraction from the attraction of a mass point. In fact σ_u^2/σ_v^2 is nearly 3, and this fits in with the Oort constants referring to galactic attraction, which are inferred from observations of differential galactic rotation.

Some of the velocity ellipses are tilted; in particular, that of the A stars. This is clearly further evidence of the fact that there is as yet no equipartition, particularly as the A stars are the youngest class for which the velocity ellipse can be drawn. It seems fair to conclude that the tilt in the velocity ellipse of the A stars is connected with the formation of A stars in a special arm of the galaxy, a spiral arm in fact.

The motion of the stars in the galaxy does not take place in a plane, as is the case for motion about a point centre of attraction. On account of the flattened disc-like shape of the galaxy, the departure from motion in a plane is considerable and it is usual to suppose that motion perpendicular to the galactic plane can be considered independently of motion in the other two co-ordinates. Each star then oscillates through the galactic plane (in the z direction) and the velocities perpendicular to the plane (the w velocities) are an indication of the attraction perpendicular to the plane; that is to say, they measure the local density.

The z motion is not simple harmonic motion, but it is easy to see that the faster a star is going upwards when it passes through the centre of the attraction the further out it will get before attraction stops it and it falls back; equally it is easy to see that for a given velocity at the central plane, the greater the attraction, the less the star will go. It is a consequence of the mathematical relations that if the velocity distribution at the central plane is a Gaussian distribution, the velocity distribution is everywhere Gaussian and with the same mean speed. All that changes is the density which falls away on either side of the central plane, and in the simplest case, where the material is stratified in parallel planes and where the stars all have the same mass, we find the very simple law for the distribution of star density ρ with distance z from the galactic plane

$$\rho/\rho_0=\operatorname{sech}^2 z/l\sqrt{2}$$

where l is a length dependent on the central density ρ_0 and the dispersion σ_w of the w velocities perpendicular to the plane, in fact

$$l = \sigma_w/(4\pi\Gamma\rho_0)^{1/2}$$

Γ being the constant of gravitation. The easiest way to make use of this relation is to examine the motions of A type stars in the two galactic caps: that is to say, A stars which appear to us in directions perpendicular to the Milky Way. The North Galactic Pole is in Coma Berenices, not far from Arcturus, and well observable in this country, while the South Galactic Cap is in Sculptor, well placed for observation from South Africa. The A stars are a good spectral type for this work because they are not divided into giants and dwarfs, and statistically speaking their distances can be found reliably from their apparent magnitudes.

Counts of A type stars, of different apparent magnitudes, near the two galactic poles indicate that the length l is about 150 parsecs.

The w velocities very close to the two galactic poles are of course radial velocities. Those in the Northern cap have been observed down to the 6th magnitude, and less completely for one or two magnitudes fainter.

In a discussion conducted in 1956 I supposed that the dispersion of the velocities was 9·5 km./sec. This leads to a local density ρ_0 of 0·18 solar masses per cubic parsec. Recently Dr. Wayman of the Royal Greenwich Observatory staff has returned from South Africa where he spent three years observing with the 74-in. telescope of the Radcliffe Observatory at Pretoria. Amongst other things he made a survey of A type star radial velocities near the South Galactic Pole. The reduction of his observations will give an improved value for the velocity dispersion and will lead to an improved value of the local density. The interesting property of the density deduced in this way is that it is total and includes besides the mass gathered together into stars the mass still in the form of gas and dust. By assessing the density due to actual stars we can by subtraction from the total estimate the mass still in the form of dust and gas. On current assessments of the local densities, an appreciable fraction, perhaps 50 per cent, of the local attraction is due to dust and gas, although the figure for the galaxy as a whole may be much smaller. This of course fits in with the general notion that the solar neighbourhood is in the outer part of our galaxy, and that dust and gas are most found in the outer parts of spiral galaxies.

It was suggested by Stromberg in 1922 that a number of stars well removed from the Hyades share its motion, and similarly it has been suggested that a number of stars including Sirius have the same motion in space as some of the bright stars in Ursa Major. Many papers have been written discussing the reality of these two groups. Recently Dr. Eggen of the Royal Greenwich Observatory has taken up the question afresh, and suggested the existence of new groups. The importance of this topic is two-fold. Firstly, if a group of stars can be shown to have parallel velocity, and the magnitude of that velocity is known from the motion of one or two of the best observed stars in the group, the parallaxes of all the stars may be found from their proper motions. The group as a whole has a radiant—in the case of the extended Hyades group it is simply the radiant of the Hyades proper. The angular distance λ of each star from this radiant may be computed easily. If V is the velocity of the group, the tangential velocity of a particular star is $T = V \sin\lambda$. But if μ is the proper motion and p the parallax, $T = \dfrac{\kappa\mu}{p}$ where κ is a known constant. Accordingly for each star

$$\frac{\kappa\mu}{p} = V \sin\lambda$$

an equation which determines the parallax p when μ, V and λ are known. Eggen calls parallaxes determined in this way *group parallaxes*.

For example, Eggen has published with A. R. Sandage an account of a group of stars moving with a faint star known as Groombridge 1830. This star has a large trigonometrical parallax, namely 0″·116, and a large proper motion, 7″·04 per year. Its radial velocity is −98 km./sec. The velocities in the galactic rectangular co-ordinate system already described are $u = -263$ km./sec., $v = -151$ km./sec. and $w = -22$ km./sec. These are large velocities. A search through a catalogue of high velocity stars yielded four stars which showed parallel velocity with Groombridge 1830, judged by the direction of the proper motion and the radial velocity. The important star RR Lyrae

is one of these; and its group parallax is 0″·0035. It is clearly much too small to be determined trigonometrically, and the result is of great importance, on account of the use made of RR Lyrae variables in determining distances. A second point of importance in the Groombridge 1830 group is that the remaining members of the group are classified spectroscopically as sub-dwarfs. These stars lie well below the ordinary main sequence, such as that of the Hyades, and Eggen and Sandage find that they lie on the HR diagram of a particular globular cluster, Messier 13.

Interesting theoretical questions are raised by the idea that groups of stars persist for a time comparable with the period of revolution round the galaxy, if the stars concerned are at appreciable distances from one another. It is clear that a group of stars formed with slightly different initial conditions of velocity and distance from the centre of the galaxy must in time spread out in galactic longitude unless the periods of the galactic orbits of the stars are equal. Either the star groups started off initially in equiperiodic orbits for a particular reason associated with the mode of their formation, or they are fragments of much larger groups of which we see in our neighbourhood a selection—selected from the large group by possessing a particular set of nearly equal periods.

The last topic to which I will refer is the determination of the distances of Cepheid variables from their proper motions. Cepheid variables—that is to say, stars which exhibit a periodic variation in their luminosity in the manner of the type star δ Cephei—occur in the Large Magellanic Cloud. In, for example, the Large Magellanic Cloud all the stars are sensibly at the same distance from us, and therefore the apparent magnitudes only differ from the absolute magnitudes by a single added constant, the modulus of the Cloud. It is well known that the Cepheids in the LMC exhibit a period luminosity relation. Fig. 7 shows the relation as recently determined at the Royal Greenwich Observatory, Herstmonceux, from plates exposed at the Royal Observatory at the Cape of Good Hope. If we knew the distances of nearby Cepheids, we could state the absolute magnitude that corresponds to any particular period, and find the modulus and therefore the distance of the Large Megellanic Cloud, and of course of any object that contains Cepheid variables.

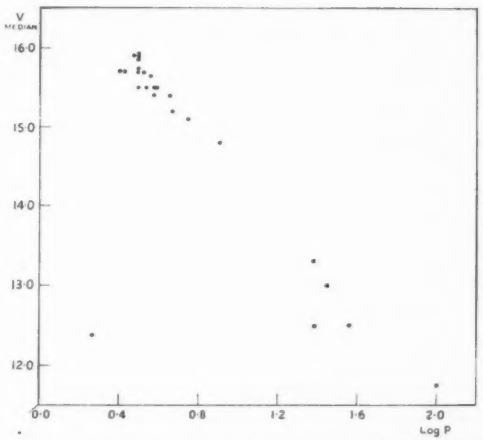


Fig.7.—Period-luminosity] relation for Cepheids in the Large Magellanic Cloud (visual).

The determination of the zero point of the Cepheid period-luminosity relation is clearly a matter of the utmost importance as it sets the scale of the entire cosmos.

Unfortunately no Cepheid variable occurs so close to the Sun that a reliable trigonometrical parallax can be measured. Indeed the closest Cepheid is the Pole Star, α Ursae Minoris, which is about 100 parsecs away. The measures of its trigonometrical parallax are shown in the accompanying Table.

TRIGONOMETRICAL PARALLAX OF POLARIS

Allegheny Observatory	0″·004 ± 0·005
McCormick Observatory	−0″·004 ± 0·010
Greenwich Observatory	0″·008 ± 0·009
Washburn Observatory (Meridian)	0″·017 ± 0·035

It is clear that we can expect nothing from trigonometrical parallaxes such as this—and Polaris is the closest of the Cepheids. But the periods are all known, and we may suppose that the stars follow the period luminosity relation found in the L.M.C.; all that is unknown is the zero point of this relation. With any zero point that we choose for trial we can work out the distances, and then we can use the proper motions to construct tangential motions in kilometres per second. Of course these results will be proportional to the

trial value of the standard distance assumed. They can be combined with radial velocities—about which there is no difficulty—to form velocity components along particular axes.

Each star has a motion compounded out of its own peculiar motion and the Sun's peculiar motion. From the radial velocities alone we can say that the dispersion of the Cepheid velocities is about 10 km./sec. As this is comparable with the solar motion we can say nothing about the motion that we find for any particular Cepheid but we ought to be able to find the reflection of the solar motion if we take the average over a number of Cepheids. This can be done in a variety of ways but perhaps the most satisfactory is to examine the motion perpendicular to the galactic plane. The problem is a difficult one, as the proper motions are all small, and it is important to free them from systematic error. However, the principal cause of dispersion in the velocities as measured is the real dispersion of the stars' motion, at any rate for one or two score of nearby Cepheids, and it should be possible to obtain a reliable zero for the period luminosity relation. A solution has been made on several occasions. As estimates of the zero point of the period luminosity relation range over a whole magnitude, the matter cannot yet be regarded as settled and it is intended to go into the question again with fresh material at the Royal Greenwich Observatory. A reliable determination of the zero point of the Cepheid period luminosity relation, carrying as it does a scale of the largest distances in the Universe, is one of the most important results that can flow from the study of stellar motions.

POPULATION GROWTH, FERTILITY AND POPULATION POLICY *

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When the new Sociology Section was established last year, it was suggested that 'population' might conveniently form the subject of the introductory address. It would fit in with the main theme at this year's meetings of the British Association—food needs and possibilities—and also provide an opportunity for a joint discussion with the Economics Section. I had not then expected that I should be taking part in the symposium on world food and population and, in consequence, dealing with some of the matters which it had been intended to discuss here. To go over the same ground again now would clearly be inappropriate. But there are some questions—especially those relating to the spread of birth control—on which I was inevitably rather dogmatic in the symposium. It may be of interest to look more closely at those questions now, especially as it is in connection with them that demography, economics and sociology need to come together in the analysis.

The questions with which we are concerned are those of today and of the short-run future. Yet we can scarcely consider them without at least a glance back towards the past. Our speculations on changes in the level of fertility in Asia and Africa should not assume that we have no experience of the spread of the small-family pattern in industrialised societies. And in the current debate on underdeveloped areas, arguments for and attacks on birth control as a means of solving the problem of rapid population growth are so often coupled with the name of Malthus and with the term 'Malthusian', that we need some historical perspective if we are to judge the relevance of Malthus and of birth control to present reality. That is my excuse for beginning with Malthus and the nineteenth century rather than plunging directly into a discussion of the 1960's.

When Malthus wrote, he and his contemporaries were concerned mainly with what was happening, or was likely to happen, in the West. It is true that, especially in the second and later editions of his *Essay*, Malthus attempted to provide a world survey and that, in any case, he was endeavouring to formulate a principle of universal validity in time and space. But the material on Asia and Africa was, so to speak, used as a 'horrid example' of the checks of 'misery' and 'vice'. The practical recommendations were derived from and related to Western experience and, in particular, to Britain.

Leaving aside the question of national interest, and considering the situation simply in terms of relative population increase, the focus upon the West is understandable. Though in fact Malthus himself had little in the way of reliable comparative data to draw upon, subsequent studies have shown that it was only in the West that rates of population growth were relatively high in the first half of the nineteenth century. In Asia, population increase was only half as rapid and it was unlikely to rise significantly since, apart from the elimination of famine and smallpox, there was no obvious way of reducing civil mortality. With expectations of life at birth of less than thirty years in many areas—in India, for example, only about twenty-five years even in the late nineteenth century—increase in numbers would inevitably be slow and halting.† In the event, population growth in Asia was at the rate of about 0·4 per cent per year in the nineteenth century, requiring a period of 175 years for numbers to double. The rate of increase in Africa was very probably still lower.

But even in Europe, where attention was mainly

* Address delivered to Section N (Sociology) on September 6, 1960, at the Cardiff Meeting of the British Association.

† Unless otherwise specified, the demographic data referred to in this paper are from D. V. Glass and E. Grebenik, 'World Population, 1800 to 1950', to be published in the final volume of the *Cambridge Economic History*.

directed, growth rates were variable, both as between countries and between successive decades in the first half of the nineteenth century. By the time of Malthus's death, expectation of life at birth was beginning to approach forty years in Britain, promising an ultimate increase in numbers of about 30 per cent or more per generation with the then current fertility. But this was exceptional. For the continent as a whole, actual numerical growth was at the rate of 0·7 per cent annually, requiring a period of a hundred years for doubling to take place. The difference between actual and potential rates of growth—between doubling in a hundred years and doubling in twenty-five—was not only wide but could not in fact have been closed very quickly with the techniques for controlling mortality available in Malthus's times. In most Western countries, it was not until the turn of the century that expectations of life at birth approached fifty years and that the fall in mortality began to accelerate. Apart from the fact that, in many European countries, the basis for fairly rapid technological advance already existed in the early nineteenth century, and hence also the capacity to absorb and profit from population increase, the prospects of reaching the physiological upper limits of population increase by reducing mortality were small or, at least, distant. And when the distance had been travelled, another factor came to enter, the control of fertility within marriage.

In Malthus's period, therefore, both actual and prospective population growth were highest in what were then the technologically most advanced societies. And even there, in general, rates of increase were well below the hypothetical upper limit. They were in fact rather more in keeping with Malthus's own estimates of acceptable growth—for he was in favour of slow growth, not of stationary or declining populations—than with the theoretical possibility which constituted one of the two spectres haunting Europe.

Today the situation is very different. In Europe, growth rates are not much higher than they were in the early nineteenth century—0·9 per cent annually instead of 0·7 per cent—the great fall in mortality having been almost counterblanced by a fall in family size.* High rates are found in two contrasted types of area: in a few industrialised countries, notably North America and Oceania and also the U.S.S.R.; and in the underdeveloped regions, whether fairly thinly peopled like Latin America, or with already dense populations like much of Asia. So far as the first group of countries is concerned, little fear is expressed about the capacity of the economies to carry continued increases of numbers. For North America, present natural increase (1951–5) amounts to 1·6 per cent annually,† far higher than in nineteenth century Europe. The implications of current trends in the U.S.A. are indicated by a recent population projection, suggesting that even with no immigration total numbers might grow from 185 to 298 millions between 1960 and the end of the century.‡ But the voice of the Malthusian is not very widely heard in the U.S.A. in respect of that prospective increase. There is discussion of the loss of amenity, of the spread of Megalopolis, of an excessive consumption of minerals and fuel significant for the industrial development of other countries. Occasionally, one also comes across biological analogies, suggesting that high population density in human communities may, as in the case of rats, produce nervous disorders. In general, however, it is assumed—and not entirely without reason—that continued population growth in the U.S.A. will at least not lower levels of living, even if they are prevented from rising as much as might otherwise be the case.

It is with the other group of high-growth countries that concern arises. They, too, are showing a natural increase of around 1·6 per cent or more—considerably more in the case of Latin America.§

* This is not to suggest that there are no important differentials within Europe. But the problems of individual European countries are of a different order from the problems of, say, the Caribbean countries or India.

† United Nations, *The Future Growth of World Population*, New York, 1958, p. 32 (U.N. 1).

‡ R. Freedman, P. K. Whelpton and A. A. Campbell, *Family Planning, Sterility, and Population Growth*, New York 1959, p. 380. The projection assumes 3.05 births per ever-married woman; 94 per cent women ever-married by 45–49 years of age; a median age at childbirth of 24·9 years; and expectation of life rising to 71·3 years for males and 77·1 years for females by the end of the century.

§ Present rates are very approximate, because of poor basic data. Latin America shows 2·4 per cent (1951–5); Africa, 2·0; Asia, 1·7. Population models suggest rather lower rates, 2·1, 1·4, and 1·3 respectively. See U.N. 1, p. 32.

They have crude birth rates several points higher than obtained in most European countries in the early nineteenth century, not because of substantially larger family size but because marriage (whether legal or *de facto*) is both early and almost universal. Taking India and Ceylon as fairly typical of a large part of Asia, for example, well over two-thirds of the women will have married by their twenty-fifth birthday and over 95 per cent by their fiftieth birthday.* The high crude birth rates have now been translated into high rates of natural increase, not because of any change in fertility but because, often independently of any alteration in levels of subsistence, mortality has come under control. In some cases—in island communities like Ceylon or Mauritius—the reduction in mortality has been dramatic, raising expectations of life at birth from underdeveloped to developed standards in twenty years or less. But even moderate reductions can shift the balance of growth when family size is somewhat above six live births to a marriage of completed fertility (as in India) and when the initial expectation of life at birth is only around twenty-five years. Completed fertility as such has not risen and is not likely to rise, except in so far as health improvements reduce involuntary infertility or convert stillbirths into live births.† Present levels of completed marital fertility are not very different from those in most Western countries during the period of industrialisation, and are about as high as can be found in large communities.‡

The present annual growth rates of 1·6 per cent or more in such countries may become still higher in the next twenty years as a result of closer control of mortality. Many areas in Asia and Africa still have expectations of life at birth of below forty years, and many areas in Latin America have not much more than forty-five years. But the possibilities of rapid improvement are very considerable. What was inconceivable in the early nineteenth century is now fairly easily within reach, and especially so far as tropical, underdeveloped countries are concerned. Their populations are still heavily affected by diseases which, even if they do not kill, greatly lower general health and increase the chances of death from other causes. Some 250 million people live in malarious areas and are exposed to both a debilitating and a killing disease. Perhaps 150 million people today are infested with bilharzia, and over 250 millions are affected by the filariases. Trachoma and infective conjunctivitis may affect some 400 million people, and there are perhaps 50 million cases of yaws in the present world.§ Tuberculosis is almost everywhere in underdeveloped countries, a dominant factor in mortality. Yet developments since World War II show how quickly well-organised programmes of public health can reduce the damage from many of these diseases. In Ceylon, between 1942 and 1954, it is estimated that the number of malaria cases fell from over 3 million to under 40 thousand.‖ In Mexico, between 1950 and 1959, malaria mortality fell from 90 to under 10 per 100,000 population. Spraying programmes have been greatly stepped up during the past few years, and the aim in the third five-year plan for India, to eradicate malaria, by interrupting the cycle of 'parasite to mosquito to human being' is by no means impractical. Smallpox control is now also becoming much more effective, mass vaccination having been carried out in a number of endemic areas. Thus it is estimated that in East Pakistan, an area in which epidemics have occurred repeatedly, 30 millions out of a

* Western societies are now also showing tendencies in this direction. In Britain, for example, there have been major changes in marriage frequency and marriage age during the past twenty years.

† Of course social change may affect the proportions of women actually married (e.g. by relaxing the hostility to the remarriage of widows). Equally, reductions in mortality will increase the probability of survival of a marriage to the end of the childbearing period and may thus raise the overall fertility rate by increasing the proportion of marriages of completed fertility. This appears to have happened among British ducal families in the early eighteenth century (see T. H. Hollingsworth, 'A demographic study of the British ducal families', *Population Studies*, July 1957, vol. XI, No. 1) and applied to Western populations as a whole in the nineteenth and twentieth centuries.

‡ In small communities, much higher levels may be found—the outstanding example being the present-day Hutterites in U.S.A. and Canada. Among them, with mortality at about the same level as for the white U.S. population as a whole, married women aged 45–54 years in 1950 had borne an average of 10·6 live births. See J. W. Eaton and A. J. Mayer, *Man's Capacity to Reproduce*, Glencoe, Illinois [1954], p. 20.

§ U.N., *Report on the World Social Situation*, New York, 1957, p. 30 *et seq.* (U.N. 2)

‖ D. M. de Silva, *Health Progress in Ceylon: a Survey*, Colombo, 1956, pp. 25–39.

population of 46 millions were vaccinated during the first half of 1958, following the outbreak of an epidemic in October 1957. Mass B.C.G. vaccination is being carried out in a number of countries, including India, to control tuberculosis.* In such circumstances, even taking into account public costs and individual apathy, the assumptions of Coale and Hoover, involving an increase in the expectation of life at birth in India from thirty-two years in 1951 to fifty-two years in 1981 are far from unrealistic.† A change of this magnitude, other things being equal, would raise the replacement rate of the population of India to 2 or more—that is, implying an ultimate doubling of numbers in a generation. Similar results are suggested by the U.N. medium population projections, which assume constant fertility up to 1975 and declining mortality (save in Middle Africa). By 1975, on these assumptions, natural increase would amount to 1·7 per cent annually in Africa, 2·3 per cent in Asia and 2·8 per cent in Latin America.‡ At 2·8 per cent increase per year, a population would double itself in about twenty-five years. We should, at such a point, be up to the Malthusian rates of growth. But if rates of this magnitude do apply, the circumstances and policy implications will be very different from those considered by Malthus. It is to these differences that I should now like to turn.

Many of the present-day references to Malthusianism bear little, if any, resemblance to the views or recommendations of Malthus. It is, of course, true, that the successive editions of his *Essay* contain so many glosses and afterthoughts that a 'total theory' constructed out of all he had written would give a far more considered and comprehensive approach than Malthus showed at any one point of time.§ Moreover, he cannot be absolved from failing to correct, or publicly to protest at, the excesses of some of his followers. This is especially so since he was ready to go into print—his pamphlet in reply to Whitbread's Bill is an example—when it was a question of correcting the opinions of those who did not recognise the power of the principle of population and of reaffirming his own view that a system of poor relief based upon the assumption that the poor have an inalienable claim to assistance was bound to be self-defeating.‖ In a late edition of his *Essay*, Malthus admitted that, in an effort to straighten the bow from the warped position in which he had found it, he might well have bent it too much in the reverse direction.¶

Nevertheless, he was neither the misanthrope nor the prophet of doom depicted in the writings of some of his contemporaries and in some of ours. He did not regard 'misery'—famine and pestilence—as a desirable way of checking population growth. On the contrary, he asserted that a low infant and child mortality was to be aimed at and would constitute an index of social progress.** He was an advocate of mass education, not a supporter of continued mass ignorance.†† Far from wishing to see populations become stationary or decline, he believed essentially in the exhortation, 'be fruitful and multiply'; it was the *speed* of multiplication with which he was concerned, taking it for granted that multiplication as such would be assured by a largely unchanging passion between the sexes. And the 'tendency' of population to grow faster than the means of subsistence was not an unmixed evil. Population growth needed to be controlled, but it was the very tendency of population to press on the means of subsistence which acted as a spur to individual and social progress. That was one of the two reasons why Malthus opposed birth control—because he believed that the use of such means

* See *W.H.O. Chronicle*, August 1960, pp. 297–303 and pp. 314–17.

† A. J. Coale and E. M. Hoover, *Population Growth and Economic Development in Low-Income Countries*, Princeton, 1958, p. 38 and pp. 62–71.

‡ U.N. 1, p. 32. On these assumptions, natural increase rates in some countries (e.g. in Asia, Cambodia, Ceylon, Taiwan, Malaya, the Philippines and Thailand) would be at or above 3 per cent per year in 1975–80. See U.N., *Economic Bulletin for Asia and the Far East*, vol. X, No. 1, June 1959, pp. 20–27. (E.C.A.F.E., 1)

§ In a sense, his final view was expressed in *A Summary View of the Principle of Population*, London, 1830. But this was published long after he had made his main impact on his contemporaries.

‖ *A Letter to Samuel Whitbread, Esq., M.P.*, London, 1807.

¶ 5th edn. London, 1817, vol. 3, p. 427.

** *Essay*, 5th edn. 1817, vol. 3, p. 336. 'I have stated expressly that a decrease of mortality at all ages is what we ought chiefly to aim at; and as the best criterion of happiness and good government, instead of the largeness of the proportion of births, which was the usual mode of judging, I have proposed the smallness of the proportion dying under the age of puberty.'

†† See, e.g., his remarks in his *Letter* to Whitbread, pp. 34–36.

would 'remove a necessary stimulus to industry'.* Instead, he recommended 'moral restraint'—a voluntary delaying of marriage until the support of a family could be assured, with a strictly 'moral' conduct before marriage and an uncontrolled (or, at least, not artificially controlled) fertility within marriage.

As was suggested earlier, Malthus was concerned particularly with a society which, technologically speaking, was already very advanced in comparison with most other societies of his period, though the lines of future development were by no means clearly visible. Levels of living had risen during the previous hundred years—apart from possible spontaneous changes in the incidence and virulence of epidemics, there is little other explanation of the fall in mortality which probably occurred in the late seventeenth or early eighteenth centuries; it could not be accounted for by medical advance.† The rate of natural increase had risen—the early nineteenth century rate in Britain must have been a fairly new phenomenon. Population pressure might act as a stimulus, but continued growth at the new high level might well seem to threaten existing or prospective levels of living when further technological advance was not guaranteed. Marriage was already in some degree a controlling variable and might become still more so, especially as marital fertility was largely uncontrolled and as there was little likelihood of additional sharp reductions in mortality. Population growth as such, even in the fairly short run, might have appeared still somewhat flexible.

But the regions in which Malthusian rates of growth may be attained by the 1980's, are not Malthus's illustrative regions of new settlement, with high returns to labour; they already have abysmally low levels of living. The increase in growth rates does not depend upon a fall in mortality following improved levels of subsistence, though it may well require improved levels if it is to persist. A substantial part of the prospective high natural increase will be unavoidable—the consequence of the existing age-structure of the populations concerned, and also the result of implementing health measures which are both entirely justified and which form part of universally accepted objectives of social progress. Marriage is not at present a controlling variable. Short-run population growth, in the absence of catastrophe, is not very flexible. The first priority of policy cannot be to operate on the rate of population growth but on the levels of living.

This conclusion is both obvious and fairly widely accepted. It underlies much of the current writing on the economics of development. While most economists probably regard rapid population growth in Asia, Africa and Latin America as a serious hindrance to raising levels of living, few would consider slow growth an absolutely indispensable prerequisite. The secretariat of the Economic Commission for Asia and the Far East, discussing the prospects of higher incomes per head in their region, say that these will 'depend primarily on the efforts made by the countries concerned to speed up and render more effective the process of capital investment and technological and organisational advance. And, in general, a greater effort will be necessary if the rate of population growth is high.'‡ So far as capital investment is concerned, it is widely assumed that a capital–output ratio of 3 to 1 would not be too unrealistic. On this basis, and taking population increase at 2·5 per cent annually, it would require capital at the rate of 13·5 per cent of national income each year to double the level of living in about thirty-five years.§ Arthur Lewis has argued that 'no nation is so poor that it cannot save 12 per

* *Essay*, 5th edn., London, 1817, vol. 3, p. 393. It is worth quoting the passage at length. '. . . I should always particularly reprobate any artificial and unnatural modes of checking population, both on account of their immorality and their tendency to remove a necessary stimulus to industry. If it were possible for each married couple to limit by a wish the number of their children, there is certainly reason to fear that the indolence of the human race would be very greatly increased; and that neither the population of individual countries, nor of the whole earth, would ever reach its natural and proper extent. But the restraints which I have recommended are quite of a different character. They are not only pointed out by reason and sanctioned by religion, but tend in the most marked manner to stimulate industry.'

† Such a change in mortality is certainly shown in Hollingsworth's study of British ducal families. On the medical 'advance', see T. McKeown and R. G. Brown, 'Medical evidence related to English population changes in the eighteenth century', *Population Studies*, November 1955, pp. 119–41.

‡ E.C.A.F.E., 1, p. 45.

§ E.C.A.F.E., 1, pp. 36–7. But these figures also indicate how a lower rate of population growth would allow a more rapid rise in levels of living. With the same investment and capital–output ratio, a growth rate of 1 per cent a year would allow a doubling of levels of living in about twenty years instead of thirty-five.

cent of its national income if it wanted to . . .', and that 'the central problem in the theory of economic growth is to understand the process by which a community is converted from being a 5 per cent to a 12 per cent saver—with all the changes in attitudes, in institutions, and in techniques which accompany this conversion'.* But in that case, half the Malthusian argument has vanished. Population no longer being an independent factor to which all other factors conform, Malthus is in a considerable measure replaced by Marx. The task of economists and sociologists is to help in remodelling the institutions and attitudes—a task which is abundantly clear, though only too complex in practice.

In effect the other half of the Malthusian argument—that concerned with principles of human conduct in the control of population growth—has also vanished. Its theoretical basis was destroyed in the nineteenth century, though the inferences in practice have only come to be accepted since World War II, and still often in partial form. Malthus advocated control through delayed marriage, rejecting birth control on grounds of morality and incentive to individual and social progress. Control through marriage might have had a significant effect in particular circumstances. In Ireland it certainly did so, though only after perhaps a million deaths associated with famine, and with the accompaniment of continued massive emigration. No real parallel occurred in England. In any case the moral basis of the argument was rejected by Francis Place. Asserting that delayed marriage itself would almost certainly provoke immorality, he substituted control within marriage for control through marriage, and launched the first birth-control propaganda. Malthus's anti-Malthusian successor at the East India College, the Reverend Richard Jones, was slightly less concerned than Place with pre-marital morality. At least, he objected to the term 'vice' which, on Malthus's definition, would be applied to 'a dozen men' who 'defer marriage till thirty-five, with a view to honourable progress to a station which they wish their families to occupy . . .', if but 'a single instance of frailty' could 'be traced to each of them. . . .'† Far more important was Richard Jones's contention that population increase is not exerted up to the limits of *subsistence*, but up to the limits of *maintenance*, maintenance being defined to cover not only food but also the commodities required by each class to satisfy 'all the wants and gratifications which are essential to maintain them in comfort and contentment'.‡ It was these non-food wants which were capable of indefinite extension. He could see 'no limit to the comforts and luxuries which human beings consider essential to their well-being and happiness, and which they will avoid sacrificing by imprudent marriages'.‖ The channel of action was the society, conditioning the levels of maintenance of the various classes and their numbers. Given this conditioning—though not wishing to see stationary populations common among 'civilized nations of great producing powers'§—he would say 'yes' to the question: 'Can elevation of habits proceed to an extent which will control the tendency of a people to increase their numbers, they having the means to do so?'¶ Indeed, there seemed to be 'no reason why abstinence, founded on plenty and on multiplied enjoyments, should not bring the progress of numbers to a stand-still'.** But if it is levels of maintenance, capable of indefinite increase, which act as a brake upon population growth, the incentive basis for claiming that delayed marriage is the only acceptable form of control no longer applies.

Developments in the past hundred years have justified Jones as against Malthus. And they have also shown the acceptance of Place's view of morality, though rather more slowly so far as public canons are concerned. In England, for example, the Non-Conformist Churches began to modify their

* W. A. Lewis, *The Theory of Economic Growth*, pp. 236 and 225–6 (cited from Coale and Hoover, *op cit.*, pp. 148 and 154).

† W. Whewell [and J. Cazenove], ed., *Literary Remains, Consisting of Lectures and Tracts on Political Economy, of the late Rev. Richard Jones*, London, 1859, p. 96. Malthus himself did not appear to be too horrified by such 'frailty', even though he called it 'vice.' He obviously regarded it as almost inevitable, recognised that it might increase with delayed marriage, but believed that such delay would still be beneficial on balance, for if it were to take place 'without any marked and decided increase of a vicious intercourse with the sex, the happiness of society will evidently be promoted by it . . .' *Essay*, 2nd edn., London, 1803, p. 599.

‡ *Ibid.*, p. 99. His alternative definition of the means of maintenance was (p. 468): 'All the means that any rank of the community require to satisfy the wants and tastes, the satisfaction of which they believe essential to their respectability or comfort. . . .'

‖ *Ibid.*, p. 102.

§ *Ibid.*, p. 537.

¶ *Ibid.*, p. 472.

** *Ibid.*, p. 536.

stated attitudes in the 1890's. But the Church of England, when it first came to discuss the subject at the Lambeth Conference of 1908, contended that to marry with the intention of practising birth control was to 'deprave the ideal of marriage', and the Bishops recommended the prohibition of all birth-control appliances and the 'prosecution of all who publicly and professionally assist preventive methods'.* It was not until the 1958 Conference that a positive acceptance of birth control was unanimously adopted.† The attitude of the Catholic Church to any form of control other than abstinence did not begin to change until after Ogino and Knaus had shown the possibility of a purely 'natural' method. Even then, and in spite of the publication of books by Catholic authors, recommending the rhythm method as acceptable, it was not until 1951 that formal recognition was given to it as the one additional, legitimate means of limiting family size.‡ Private morality, however, changed more rapidly. In Britain, among the couples married in 1935–9 over 70 per cent were practising or had practised birth control, including at least 46 per cent of the Catholics.‖ Surveys of more recent marriage cohorts in the U.S.A. show a somewhat higher incidence in that country among the white population as a whole, with over a quarter of all regular church-going Catholic wives using prohibited methods.§ The indirect evidence indicates that contraception is firmly established in most other Western societies, with or without sanction by the majority Church and sometimes in the face of very hostile legislation. It is also clear that the main spread of birth control was not greatly assisted either by new techniques or by birth-control clinics. New techniques were available, but were little used. Birth-control clinics, even where they were permitted or encouraged, had a very small direct impact and, in any case, were not established until after the most important first stages of the emergence of a small-family pattern had been achieved. In the Western world, the institutional provisions for birth-control advice, and to a considerable extent the invention of newer techniques of contraception, display and confirm the dominance of family limitation; they do not explain it. Whatever the immediate mechanisms of personal involvement, the underlying basis of new family norms must have been changes in the levels of maintenance, and these changes took time to effect and for their reflection to be shown in reproductive behaviour.

How far can Western experience be short-circuited for underdeveloped societies and the control of fertility be achieved in a relatively short period, thus easing the task of economic transformation? The prospect does not seem too encouraging. It is true that some of the obstacles encountered in the West are likely to be less important now—the religious obstacles, for example—and that some governments have now adopted policies for spreading birth-control information and for providing much easier access to contraceptives. In aid of these policies there are now research techniques for assessing the interest of married couples in birth control, for testing the acceptability of various types of contraceptive and for measuring the impact of different approaches to married couples. Married couples will in any case be approached in a much more direct and positive way than could have been contemplated in the West in the nineteenth century. And contraceptives themselves are likely to become far more acceptable and effective now that the relevant research is coming to be regarded as 'respectable'. Nevertheless, it is easy to overestimate the significance of these new circumstances.

* *Conference of Bishops of the Anglican Communion, 1908*, London, 1908, pp. 145–7.

† See F. Campbell, 'Birth control and the Christian Churches', *Population Studies*, November, 1960.

‡ Concern for the economic and social problems associated with rapid population growth has spread Catholic discussion of the use of the safe period. See G. H. L. Zeegers, 'Introduction to the international contest on the population problem of underdeveloped areas', *Social Compass*, June 1955, and S. de Lestapis, S. J., *La limitation des naissances*, Paris, 1959.

‖ E. Lewis-Faning, *Report on an Enquiry into Family Limitation* . . (Papers of the Royal Commission on Population, vol. 1), London, 1949, p. 81, Table 60. The figure of over 70 per cent represents a correction of Lewis-Faning's data to allow for incomplete exposure. The uncorrected figure is given for Roman Catholics; a corrected estimate might amount to around 50 per cent.

§ Freedman, Whelpton and Campbell, *op. cit.*, p 184 and Appendix E, pp. 469–77. For all white women acknowledging the use of contraception, the proportion is 70 per cent; but allowance should be made for the contraceptive effect of douching. Including those who use douching for cleanliness only, the proportion rises to 81 per cent. Among Catholic women, the admitted users of contraception amounted to 57 per cent, compared with 75 per cent for Protestants.

Thus, though the major religions in Asia do not present an actively hostile face to contraception and the small family—and in any case their ideologies are capable of reinterpretation—they are not positive and their exponents—unlike those of the Christian Churches—have not hitherto occupied themselves with the kinds of questions involved. And there are interpretations—such as Gandhi's belief in continence as the only acceptable form of control—which have had a considerable impact on public and private opinion.* Hence in part the attempts in India to begin with the rhythm method rather than with contraceptives. So far as Latin America is concerned, the acceptance of the rhythm method by the Catholic Church does not necessarily mean that priests will campaign on its behalf. Moreover, the new attitudes *are* new, and will have to contend with the backlash of traditional views forcibly expressed until only a short while ago. As recently as 1945, in Barbados, for example, a senior government official stated that 'so far, no civilised country has been prepared to accept birth control in general application, and it is not reasonable to expect any Christian people to agree to this crude method of reducing the population'.† The present objectives of emigration and birth control may well seem a little sudden.

Where family limitation is adopted as part of general policy or accepted as socially and economically relevant, much will depend upon the effective availability of advice. In relatively small communities, this may not require many clinics or other agencies. In Hong Kong, for example, clinics are held in nineteen areas, and the new cases seen in 1958 were equal to about 8 per cent of the registered births in the city. The Family Planning Association estimates that the work of the clinics may have brought the crude birth rate down from a possible 41·1 to a recorded 38·8 per 1000 in 1958.‡ Not all small communities can claim even that contribution. In Barbados the first family-planning clinic was opened in 1955 and additional clinics in 1956. The clinics were given increasing government grants, though there was antagonism from the Anglican Bishop and from the political opposition. A report produced in 1957 by a U.S. technical expert invited to look at the programme concluded that, if it were to continue at its existing pace, the birth rate would be lowered by 1 per cent in twenty years or more, assuming that, after attendance at a clinic, women reduced their rate of childbearing by a third.§ Large societies, of course, have to face the need for training very considerable numbers of clinic and other suitable assistants and for establishing sufficient numbers of clinics (or other devices) to assure reasonable accessibility for their populations. The size of such a task for a country like India is not difficult to imagine. And though a significant beginning was made during the second Five Year Plan, there were by the end of 1958 only 826 family-planning clinics and 1316 maternity and child health centres giving advice on contraception for a country with a population of about 400 millions among whom there was little sign of a spontaneous fall in fertility.‖ Moreover, theoretical availability of advice and materials is far from being the important question. Thus in Puerto Rico, with a population of 2·7 millions, advice and materials are theoretically available from all the island's 160 public health units. But relatively little emphasis is placed upon, or publicity given to, this side of the work of the units, and health rather than birth

* On Buddhism in Ceylon, see B. Ryan, 'Hinayana Buddhism and family planning in Ceylon', in Milbank Memorial Fund, *The Interrelations of Demographic, Economic and Social Problems in Selected Underdeveloped Areas*, New York, 1954. On India, I am indebted to Mrs. Thapar, a graduate student at L.S.E., for the provision of unpublished material. Both Gandhi and Mrs. Annie Besant, honoured as leaders of the movement for independence, believed in continence (Mrs. Besant abandoned her support of birth control when she became a Theosophist). See also N. V. Sovani, 'The problem of fertility control in India: cultural factors and development of policy', in Milbank Memorial Fund, *Approaches to Problems of High Fertility in Agrarian Societies*, New York, 1952, pp. 63–73. On the views of Moslem religious scholars in Egypt that birth control is fully compatible with Islam, see Hanna Rizk, 'Population policies in Egypt', in *Fifth International Conference on Planned Parenthood, 1955*, London, n.d. pp. 38–40 (*I.P.P.F.*, 55).

† Cited by D. Lowenthal, 'The population of Barbados', *Social and Economic Studies*, Dec. 1957, p. 488.

‡ M. Chidell, in *Sixth International Conference on Planned Parenthood, 1959*, London, n.d., pp. 348–351 (*I.P.P.F., 59*). The Hong Kong Family Planning Association began its post-war work in 1951.

§ C. Tietze, in D. Lowenthal, *op. cit.*, pp. 489–92.

‖ B. L. Raina, 'Family planning programme in India,' *I.P.P.F. 59*, pp. 285–287.

control has been stressed since 1952. Only just over a fifth of the least educated rural population know of the availability of the service and less than one family in ten in Puerto Rico has ever obtained contraceptives from the units.*

During the past ten years, there has been a marked increase in the number of direct studies in underdeveloped countries of attitudes to family size and birth control and, more recently still, of experiments in the actual spread of contraception. To discuss the results of even the most important of these new researches would require at least an additional lecture. But it is worth noting here a few of the more general findings, especially of the studies carried out in India. First, sample investigations have shown that there is very little hostility to the idea of family limitation among urban or rural populations. On the contrary, in some studies substantial proportions of husbands and wives stated that it was desirable to limit the size of the family. In the cities of Lucknow and Gorakhpur, for example, 46 per cent and 54 per cent respectively of the husbands expressed this attitude.† In a study in six rural communities in Bombay State and Hyderabad, 39 per cent of wives aged 20–45 showed a 'favourable attitude' to contraception.‡ One of the most recent investigations, an inquiry into an incidence of contraceptive practice in Calcutta in 1956–7, shows high proportions of married couples who were practising, or had practised, some form of birth control. Among couples married less than five years, for example, the proportions 'ever-practising' were 82 per cent for the higher professional group, 65 per cent for the middle class and 35 per cent for the manual workers.§ Even this last figure, low though it may seem, is higher than might have been expected. At first sight, then, these various studies would appear to suggest a readiness to limit family size, capable of being translated without too much difficulty into new reproductive patterns. In fact, however, the same studies indicate that this is far from being the case. A favourable attitude—or, rather, an unhostile attitude—to family limitation is by no means the same as a positive one, and positive attitudes, as in more developed societies, tend to be found mainly among the better educated and the less traditionally oriented couples—those, for example, for whom the joint family with its warmth and comprehensiveness has less attraction and who are more concerned with the education of women. At present there are relatively small numbers of such couples. Or a fairly positive desire to limit family size may be shown by couples who have already reached their targets of live-born or surviving children—or, perhaps still more important, of sons. But the target figures take into account present high mortality. It is thus not surprising that, for many couples, a concrete interest in limiting family size develops mainly after they have had four live-born children.‖ Added to these factors, which derive from the basic features of Indian society, are the problems of finding contraceptives which are acceptable and effective and of persuading couples who are sufficiently interested to begin practising birth control, that persistence is the price of effectiveness. In developed societies with a firmly established small-family pattern, these might be regarded as secondary problems. In India, where the pressures have so far been towards high rates of childbearing, they are part of the major problem of transforming attitudes.¶

Circumstances of this kind are, of course, found in other underdeveloped or semi-developed societies. In Puerto Rico, for example, there are equally apparent incompatibilities between ideal or preferred family sizes (which tend to be around two or

* R. Hill, J. M. Stycos and K. W. Back, *The Family and Birth Control: a Puerto Rican Experiment in Social Change*, Chapel Hill, 1959, pp. 116–24.

† J. N. Sinha, *Urban Fertility Patterns: a Survey in the Cities of Lucknow and Gorakhpur*, Lucknow, 1959, pp. 24 and 67 (mimeographed).

‡ K. Dandekar, *Demographic Survey of Six Rural Communities*, Poona, 1959, pp. 88–90.

§ S. M. Poti, C. R. Malaker and B. Chakravorti, 'An Enquiry into the prevalence of contraceptive practices in Calcutta City (1956–1957)', *I.P.P.F.*, *59*, pp. 52–60. See also 'An experimental survey of attitudes of Indian women to family planning: a preliminary study in the city of Calcutta and in rural areas in its vicinity,' *Monthly Public Opinion Surveys of the Indian Institute of Public Opinion*, December, 1958 (survey carried out in November and December 1958).

‖ See also the 1954 study by W. A. Morrison in a village on Bombay State, *Milbank Memorial Fund Quarterly*, July 1956, pp. 262–86 and Jan. 1957, pp. 67–81.

¶ For an interesting account of some of the problems involved in spreading birth-control practice in Indian villages, see J. E. Gordon and J. B. Wyon, 'A field study of motivation to family planning', *I.P.P.F.*, *59*, pp. 72–79. See also B. Singh, *Five Years of Family Planning in the Countryside*, Lucknow University, 1958 (mimeographed).

three children); actual use of birth control (40 per cent or more of Puerto Rican families have made some use of birth control); and the level of fertility, which was very high in 1951, though there is some indication of a fall since that time.* The Jamaican picture shows similar features.† In both societies the most recent studies have focused particularly on these incompatibilities, have looked into the role of husband-wife dominance and communication in matters of family size and have attempted to formulate programmes which attach married couples more effectively to the small-family model.‡ The new type of programme is certainly worth trying and may well have some effect in Puerto Rico which, though semi-developed, is much further on the ladder of development than most Latin-American societies. But whether such approaches would produce major results in, say, India, is far more doubtful.

This doubt may be illustrated by citing the one example of an Asian country in which effective family limitation has spread throughout the population since World War II. That country is Japan, where the crude birth rate has fallen to about 19 per 1000 and the gross reproduction rate, which was over 2 at the beginning of the war, was under 1·0 in 1957—lower than in England and Wales or in France. This very rapid decline has been associated with a change in official policy, beginning with the Eugenic Protection Law of 1948, which authorised the manufacture and sale of contraceptives, permitted sterilisation and provided for legal abortion on grounds which made both widely available as forms of birth prevention. Since 1948, official attitudes have become more positively in favour of contraception, both because of a more definite adoption of controlled population growth as a national policy and in order to substitute birth control for abortion, which has become the major method of limitation. For 1957, the reported number of induced abortions amounted to 1·12 millions, compared with 1·57 million live births, and it is believed that the actual number of abortions was very substantially higher.§ Repeated surveys since 1950 have, however, also shown a growing use of contraception. In 1959, among married couples in the childbearing ages (wife's age under fifty years), almost 63 per cent were or had been practising contraception, the corresponding proportion for 1950 being 29 per cent.‖ And experimental studies in Japanese villages have shown an equally marked spread, coupled with a more effective practice and with a fall in the resort to abortion.¶ There has been, since 1948, much official and voluntary assistance to the birth-control movement, and perhaps more publicity in the press than in any other society. The case of Japan would thus seem to support the view that a population can very rapidly be persuaded to change its reproductive patterns. But it is not simply a matter of mass persuasion by official and voluntary organisations.

Japan has many of the features of a traditionalist society. But the tradition itself includes the control of population. During the isolationism of the Tokugawa period, the closeness of the balance between population and resources was highly visible and birth prevention by abortion and infanticide was well-known. It was with the Meiji Restoration, and with the assumption that population growth is associated with industrial power, that efforts were made to suppress these practices. Nevertheless, in the less developed parts of the country infanticide was still occasionally practised in the 1920's and there was little feeling of 'guilt' regarding abortion. The 1920's, too, saw the beginning of a birth-control movement in Japan, and though this collided with and was suppressed during the militarism of the 1930's, evidence of changes in marriage ages and of lowered marital fertility was clear before World War II. The War itself was a shock of the order of magnitude of the 1845 famine

* Hill, Stycos and Back, *op. cit.*, pp. 106–8 and 164–75. The crude birth rate in Puerto was about 38 per 1000 in 1949–53.

† J. M. Stycos, K. Back and D. O. Mills, *Prospects for Fertility Reduction: the Jamaica Family Life Project of the Conservation Foundation*, New York, October 1957 (mimeographed).

‡ The studies referred to in footnotes * and † above.

§ There were also 44,000 cases of sterilisation in 1957. See M. Muramatsu, 'Effect of induced abortion on the reduction of births in Japan', *Milbank Memorial Fund Quarterly*, April 1960, pp. 153–66.

‖ Population Problems Research Council, *Fifth Public Opinion Survey on Birth Control in Japan*, Tokyo, 1959, p. 19.

¶ Y. Koya, 'Seven years of a family planning programme in three typical Japanese villages', *I.P.P.F., 59*, pp. 304–8. On birth control in Japan see T. Terao, *Outline of Birth Control Movement in Japan*, Japanese National Commission for U.N.E.S.C.O., Tokyo, 1959.

in Ireland. But the demographic reaction was more nearly comparable with that in Germany after World War I. For in terms of economic change, Japan was not an underdeveloped society, but technologically one of the more advanced societies. The literacy rate was one of the highest in the world; education at the primary and secondary school level was firmly and widely established; mortality had fallen to Western levels, the expectation of life at birth being around sixty-six years in the period 1950–55. Bamboo might be a characteristic of many of the houses, but the people living in them were involved in the manufacture of some of the most modern electrical and optical instruments; and it was industry which contributed the vast bulk of the net output of the country. In the 1950's, the rate of net saving amounted to some 26 per cent of the net domestic product. The underpinning of demographic change was economic transformation.*

We thus return to Richard Jones and to changes in the level of maintenance. The establishment of new demographic patterns involves the development of new incentives which press upon reproductive behaviour. Such incentives need to be imbedded in the social framework. They can be encouraged by persuasion but can hardly be created entirely or even primarily by an apparatus of symbols. Symbols are important and far too little use is at present made of them in social and economic transformation. But in countries with low levels of living, perhaps even more than elsewhere, it is necessary, so to speak, to have something material to be 'symbolic about'. Rapid economic and social change is needed if levels of living are to rise substantially in the face of high population increase. Such change is equally needed as the basis for new levels of maintenance and for the readjustment of population increase itself.

* On all aspects of Japanese demography, the standard work is Irene B. Taeuber, *The Population of Japan*, Princeton, 1958. See also her paper 'Japan's demographic transition re-examined', *Population Studies*, July 1960, pp. 28–39.

EXPERIMENT IN PSYCHOLOGY—A CENTENARY AND AN OUTLOOK*

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Custom—for once reasonably apt—attributes the beginnings of experimental psychology to the publication in 1860 of Fechner's *Elements of Psychophysics*. I cannot, with honesty, claim that my own interest in the subject extends back more than a third of that time. But perhaps I can best acknowledge the honour, and discharge the duty, you have imposed on me in 1960 by taking 1860 as a starting point, so that we may all pay tribute to the founder of our science. A hundred years in the history of science is a long time. A century ago today the *Origin of Species* had been in the bookshops less than a year. Sigmund Freud was a little boy of four, Charles Sherrington a year his junior. Even Pavlov, born in 1849, however precocious, could scarcely have taken the first steps towards the discovery of the conditioned reflex. As to those names which mark for us the period between the wars, Köhler, Lashley, Bartlett, Hull, to name but four, twenty years were to pass before even their parents were to think of them. Since 1860 there has been great, and positively accelerated, activity. Fechner himself inspired an immense quantity of work distinguished more by bulk than scientific outcome, yet it remains true that he was the originator of surprisingly much that remains central to experimental psychology as we know it today.

I would like to pick out from the story which follows Fechner's *Elements* a few samples chosen for their bearing on the question of what part experiment has to play in psychology. Views about this have differed a good deal during the past century. Some have held that the very character of the subject precludes experiment having any place at all: others that nothing can claim the status of psychological research which does not conform to the basic pattern of the conditioned reflex experiment. We need not worry with extremes such as these. But lesser degrees of doctrinaire finality are still in evidence today. As a consequence not only the worthy research student, but his elders and betters, are sometimes condemned to routines which are dreary in execution and too often trifling in result. Most psychologists nowadays will agree that experiment has a contribution to make. Many would hold that contribution to be vital. I myself would go so far as to say that psychology can only make progress in the long run in so far as it is able and willing to submit its basic propositions to empirical test in an experimental framework. But this certainly does not mean that we all ought to plunge furiously into some experimental activity or other as the only alternative to a guilty feeling that we are not doing our job. On the contrary, it seems to me, we should try to be as selective and discriminating as possible in our use of experiment. There is at least one good reason for this. Experiments are time-consuming: we need more good ones and ought not to waste time on bad. It is not easy to say what the characteristics of a good experiment are in this connection. Certainly in a subject so wide in scope, which started from scratch no more than a hundred years ago, it seems likely that there will be different sorts of 'good' experiments—and an even greater variety of 'bad'. In looking at them it will be useful to keep a number of questions in mind. First of all, to what extent and in what particular connections ought we to strive to be quantitative? Again, with regard to the relation of experiment to theory, what kinds of experiments generate useful theory, and conversely, what sort of theoretical formulation helps to build a coherent empirical structure capable of sound extension beyond any particular

* Address delivered to Section J (Psychology) on Friday, September 2, 1960, at the Cardiff Meeting of the British Association.

theory and giving rise to new connections of ideas? Finally, we shall do well to bear in mind the circumstance, humiliating but not uncommon in psychology, that having devised experiments respectably scientific and theories which interact with them, we find ourselves remote from the crude facts of everyday life which originally aroused our curiosity and which it is our aim to interpret and explain on a rational scientific basis. Scrutiny of the past will give no complete answers to questions of this kind. But it may at least tell us where some of the pitfalls lie. Let us go back a hundred years and start with Fechner.

Fechner was a professor of physics by trade, by temperament a rather unbalanced, quixotic, romantic humanist. (Fortunately before these latter tendencies gained the upper hand he had absorbed the lessons offered by the experimental study of electricity.) In an increasingly materialistic world he burned with zeal to enlarge the status of the human spirit. This, he concluded, could only be done by deciding what relationship there is between body and mind. A physicist trying to understand something material tends to measure it. Could not events in the mind also be measured, and the connection between the two established as a quantitative functional relation? Not directly, for, he writes, 'the measure only of the physical is directly accessible, so that the measure of the psychical must in the first instance be achieved through its dependence upon the former'. One fact emerges straight away from any systematic exploration of the sensations resulting from stimuli of different strengths. This fact is that two stimuli which are quantitatively distinguishable by physical measurement of their intensities are not always distinguishable by the sensations they provoke. They only become so when the physical differences reaches a certain magnitude. This finding, adumbrated by Bouguer in the mid-eighteenth century and studied in some detail by E. H. Weber in the early nineteenth, provides Fechner with his starting point. If we suppose (and I cannot discover that Fechner ever questioned this assumption) that to *every* stimulus-intensity there corresponds one and only one sensory magnitude, then we should be justified perhaps in taking the sensation interval which corresponds to the difference of any two just discriminable stimuli as constant. We could then use it as a unit of sensation. And as a measure of its strength we could allot to every sensation a number corresponding to the number of steps of just perceptibly different physical intensities from the minimum intensity at which there is no sensation at all. With this idea in mind, Fechner embarked on a study of just discriminable stimulus intensities and especially of the way in which the discriminable difference varies with the absolute value of the intensity itself. It is not easy now to be sure that we fully grasp Fechner's meaning. Many people would certainly feel that there is something fishy about his argument. Certainly it started a far-reaching but largely futile controversy over the measurement of sensation which deflected the attention of most of his successors from more profitable enterprise. Ought we then to hold up Fechner as an example of how not to go about experimenting in psychology? I do not think so. Notice how, with an intuition which perhaps he owed to his training in the physics laboratory, Fechner moves from questionable convictions about body and mind, via a shaky argument, to an experiment with a definite outcome which can be expressed wholly in terms of physical measurements. The experiment and its result have a standing independent of their *rationale*, and offer scope for the classical device of seeking functional relationships among variables. The whole procedure might still, of course, have had merely trivial results. But this did not happen. Precisely these relationships among stimulus intensities are very much the subject of active enquiry and speculation at the present time.

Trying to refine and enlarge the work started by Weber, Fechner came across a difficulty which must have been particularly tiresome to a physicist. This is the inescapably variable response of the human organism to any given stimulus. If a human being tries to decide which of two weights is the heavier by poising them in his hands, the variability of his judgments on different occasions is far greater than any variation which crops up when weighing them on a balance. How discouraging and baffling such a situation might be to a pioneer without ready-made techniques, is not easy for us to appreciate. Like the first-year student trying to apply the Constant Method, Fechner might well have been excused for concluding that there is no scope for quantitative study in this field. His pertinacity in perfecting and applying his

psychophysical methods in the face of such striking inconsistency in the observable facts stands out as a major achievement of nineteenth-century biology. As much more recent students of living organisms have realised, biology is full of cases where progress can only be made by using statistical reduction of the raw data. It is entertaining to recall that the particular mathematical device he used in connection with the Constant Method is the direct forerunner of what now goes under the name of Probit Analysis. This today is the method of choice for testing the efficacity of insecticides: the tendency of any individual insect to respond mortally to a given dose is as tiresomely unreliable as the human subject's to respond to an auditory stimulus by reporting asound.

Fechner could never have made the progress he did had he not stuck, with unwitting boldness, to the idea that there *is* a quantity to be measured, and that the variability of experimental results is attributable to the operation of numerous small random contingencies, the effects of which we can with care diminish, though not wholly eliminate. So far as I can see, he did *not* contemplate the possibility, as we sometimes do today, that the quantity itself has no more than a statistical definition in the last resort and is ultimately indeterminate. The straightforward nineteenth century physicist's view of measurable quantities stood him in good stead. The theory of errors of observation could be invoked and it allowed Fechner to arrive at most probable estimates of psychophysical variables, together with some estimate of the uncertainty of the values. It is forbidding to speculate about what might have happened had psychophysics had to start its career against a background of modern quantum theory.

When Fechner gave us the psychophysical methods he showed how purely physical measurements could be used to attack psychological problems. But he did far more than this. The framework of experiment he introduced was so fundamental that it is applicable in psychology outside the range of phenomena he was primarily concerned with. He gave us the stimulus-response method itself, and upon this practically all that we nowadays regard as experimental psychology rests. This, too, joins psychology directly on to physiology. While Fechner was composing the 'Elements of Psychophysics' his great contemporary Claude Bernard was engaged, in his laboratory, in provoking a known change in the environment of his animal preparation, noting and measuring the change of behaviour, and drawing conclusions as to the mechanism underlying the function in which he was interested. But Bernard was fortunate in being able to control the conditions of his experiment closely enough to make it repeatable within the limits required by his arguments. In experiments upon the intact human organism, on the other hand, the use of the stimulus-response method is inseparable from recourse to the experimental and statistical framework provided by Fechner.

The immediate result, however, of Fechner's efforts was to generate a controversy as to the measurability of sensation which might well have led any outside observer to conclude that the fear rather than the hope of a solution was responsible for its continuance. There are still those for whom this question, translated into suitable operational terms, is still a real issue. But for the most part the controversy itself lies buried in the dustbin of history, and is best remembered by the epitaph William James pronounced over it.

Fechner did more than start an arid controversy. He directly inspired Wilhelm Wundt. Wundt had the background, commanded the means and disposed of the compulsive energy which can do duty for enthusiasm, to make Leipzig the capital of a new empire and to set it about with an armamentarium of brass instruments. The outcome resembled that of other empires. Some who found it a centre of attraction and a fount of ideas and methods later went their pioneering ways. Others became high priests and ambassadors. In due course the weapons, originally intended for attack upon problems, became fetish objects endowed with magical powers of defending established notions. Research deteriorated into ritual. Nonetheless, Wundt's most outstanding achievement was, perhaps, the introduction of 'instrumentation' into psychology. There comes a time in the development of every science when *one* condition of success in tackling a problem is to look at it from the point of view of the instrumental resources that would be required to solve it—to consider how far they already exist and, if not, whether and how they could be devised. It is, for example, useless to frame a question whose answer would require the measurement of time to a millionth of a second

if the best means we have of measuring time is accurate only to within a thousandth. We must either reformulate the original problem so as to pose another interesting question which the clocks we have can answer or else set about devising a more accurate method of measuring time. Either outcome is likely to start new lines of enquiry. In another way Wundt's establishment of the psychological laboratory, with its acknowledged instrumentation and the identifiable forms of its inmates' activities, proved less fortunate. Rather prematurely, experimental psychologists acquired a consciousness of professional unity. Outcrops of parochialism and defensiveness against influence from outside has sometimes been the result.

Perhaps it is not accidental that the next outstanding innovator was no product of the Wundtian stable. Hermann Ebbinghaus never got closer to Leipzig than the University of Halle, disposed of no brass instruments, and worked alone. His experimental attack upon memory is truly distinguished and is in many ways a model of good experimental research in psychology. First, he picked out of what at that time was the most coherent and articulated set of ideas about the subject—namely those of the English empiricists—just what could be made the subject of precise experiment yet did not saddle him unduly with hidden presuppositions. Secondly he appreciated the need, even when trying to study the higher mental processes, to make experiment yield behavioural and quantitative data, and to use every means in his power to control the conditions. Thus, even at the risk of artificiality, he broke down the complex material with which he had to deal into small elements—the famous nonsense syllables. If not identical, these could *prima facie* be regarded as equivalent. By making these elements small, and using a lot of them, he was able to get a reasonably continuous scale in respect of quantity. This ruthless pruning and shaping of his problem, which led others to assert later that Ebbinghaus had studied only a small, and not the most interesting, part of what we mean by memory is nonetheless highly characteristic of experimental science. So cavalier a treatment of what we all know to be a rich and complex field of phenomena fully justifies itself when some simple form of relationship emerges in the results. Ebbinghaus's two most important findings, that oblivescence broadly speaking follows a course of exponential decay and that the structure of the material and not only its elements plays an important part in retention, suggestively point in opposite directions. The first conclusion might seem to bear out a view of memory which made retention dependent upon some sort of trace in the brain, some physico-chemical or physiological disequilibrium which resolved itself in course of time by a process of leakage or of tissue change. Had physiology been further advanced in Ebbinghaus's day than it was, or indeed than it is now, such a finding might have encouraged a variety of hypotheses suitable for experimental test at a physiological level. As things have turned out, Ebbinghaus's simple, provisional, experiment and conclusion have proved, after a long interval, a stimulus in other directions. They have provided a starting point on which the fresh impetus, first of Hullian conditioned reflex behaviour theory, and later of information theory could extend their hypotheses into the field of memory.

The implications of Ebbinghaus's second finding, that organisation of material is a factor in retention, seem to have been rather neglected. A fresh attack upon this aspect of remembering developed from other preoccupations. What Ebbinghaus wrote in his *Grundzüge* makes it clear that he was by no means unaware of a reconstructive aspect of recall. But it was left to Bartlett to make this feature the object of original experimental attack. Bartlett's approach, by comparison with that of Ebbinghaus, is not at all quantitative and not very systematic. But it is one which is quite indispensable at certain stages in the development of a science, notably when more limited and rigid methods have lost the initial impetus of their early success. Bartlett's methods of *Repeated Reproduction* and of *Serial Reproduction* allowed scope for the operation of complex and important factors in recall that were carefully, and from his point of view rightly, excluded by Ebbinghaus. Bartlett's investigations are related to those of the philologist tracing the evolution of the meanings and forms of words, of the archaeologist following the development of a decorative or symbolic *motif* or of the social anthropologist who tracks down the origins of myth, legend and folk-tale, rather than to the manipulations of the laboratory scientist. Successful use of methods such as these is limited to certain fields. It requires a firm grasp of their limitations and an

unusual amount of implicit discipline. Little, if any, conventional experimental scaffolding is there to prevent the work from going astray. The possibility of physiological check is remote. Bartlett's methods do not lend themselves much to further refinement in the direction of systematic quantitative experimental design. They remain somewhat esoteric, their precise character less important than the conclusions of the man who devised and used them. His clear demonstration of the reconstructive, as opposed to the merely reproductive, character of memory processes effectively put a stop to the idea that memory could be directly interpreted in terms of a conglomeration of simple independent storage processes. Effort in the laboratory was thereby led into more fruitful channels, and clinical studies of memory and language disorders refreshed. Another important—if perilous—consequence of Bartlett's work was the importation into psychology of Head's concept of the *Schema*. Perhaps the chief merit of this has been that it enables us to move with conceptual ease among a great variety of psychological phenomena, many of them very obscure, and reaching far beyond memory itself. In the fields of thinking, skill, language and perception, the navigational aids provided by older views prove inept, or would confine us to certain limited paths.

In this connection it ought to be said that the value of this new freedom of movement could prove illusory. There is a current tendency—and it is to be seen in institutes of clinical neurology as well as in psychological laboratories—to invoke the word 'schema' wherever there is difficulty in describing clearly some aspect of psychological or physiological organisation. It is no disparagement of Bartlett's personal and unique achievement to say that the word 'schema' has an important rôle in contemporary psychology largely because it stands for something of which we are ignorant. There is everything to be said for replacing worn-out myths by fresh ones, but we ought not to forget that myths can sustain complacency as well as urge action. Some people, but I am not one of them, may think that psychology has no further need for workers like Bartlett. Certainly we cannot command or contrive their appearance on the scene. The taste, the flair and the peculiar variety of empirical logic demanded by such work seem to arise before the age of intellectual training.

In two respects there is a similarity between the contributions of Bartlett and those of the Gestalt school. The latter also arose out of dissatisfaction with earlier views, especially in their atomistic aspect, and a determination to demonstrate experimentally their inadequacy. In the same way, too, it gave to psychology—indeed to the world at large—more than one useful catchword device for the denotation of the ineffable. No-one can impugn the brilliant experimental achievements of the Gestalt school nor deny the powerful impact its theoretical formulations had on the development of psychology in the first half of this century. Nonetheless the impetus eventually petered out. Why was this? Basically, it seems to me, because of an inappropriate relation between theory and experiment. Wertheimer's early work on the perception of pattern rather over-endowed Gestalt theory with terms which could not be defined on a more fundamental or unitary basis. We are still very far from being able to give any coherent account of everything we mean by the words 'form', 'shape', and 'pattern'. Wertheimer, apart from being able to draw upon conceptions deriving from geometry, such as similarity, proximity and symmetry, had to invent expressions as best he could and some of his inventions have today a rather figurative, if not metaphysical, ring. The philosopher von Ehrenfels had bequeathed him the term *Gestaltqualität*. He added such other terms as 'common fate', 'good continuation' and '*Prägnanz*'. These curiosities, especially the word *Gestalt*, served their purpose so well in articulating Wertheimer's attack on the elementaristic hypothesis that most people were convinced of the facts so brilliantly emphasised by his demonstrations. That this controversy is nowadays a matter of history, and that we tend to wonder what the fuss was all about, testifies to the distinction of Wertheimer's work.

At that time some people were intrigued by the incursion of exciting ideas like Gestalt into experimental psychology. Others, acknowledging the brilliance and interest of the great train of experiments thereby set off, remained cautious as to the terminology. Sherrington, for instance, reportedly said of Gestalt psychology, when it had acquired the rounded aspect of a school, 'Ah, it's all laid up in Heaven'. The general idea certainly stirred fresh thought in a number of fields outside the experimental. Psychopathology, neuropsychology, social

psychology and aesthetics all gained fresh mobility. In the experimental field although, especially among Köhler's research associates at Berlin, the impetus was well maintained, the theoretical framework did not encourage investigations which could transform the concepts and bring them into more direct relation with what can be physically measured. This is scarcely surprising, for they had originally been devised to allow escape from the trammels of an inadequate physiology. In a later phase, attention centred upon the concept of isomorphism. In 1921 Köhler had tried by an effort of virtuosity to bring the concept of Gestalt into direct relationship with the properties of appropriate physico-chemical systems. The austerity of his theory of *Physische Gestalten* did not altogether commend itself to his followers. They accepted the general idea of a *field*, and endowed this with convenient, if ill-defined properties. Only certain, not unsympathetic, sceptics took the idea that this field is in the brain seriously. Concrete, quantitative reference to its physico-chemical or physiological characteristics was more than discreet. Once again the theoretical formulation, and the experiments which proceed from it, did not conduce to any explicit picture of the mechanisms at work.

The birth, decline and fall of the Gestalt idea are certainly not without their lessons for contemporary psychology. To invent fresh concepts in order to gain empirical mobility is tempting, and often immediately rewarding. But disappointment may follow unless these concepts can be refined quantitatively, or in some other explicitly logical sense. Nonetheless, Gestalt psychology was a glorious revolution. Pregnant figures are no longer shameful, to be explained away—if at all—by unconscious inference!

But at the same time another and probably far more significant revolution was in progress. Behaviourism had something which Gestalt psychology did not—an origin in a physiological discovery. It also had John B. Watson, a man who expressed views so outrageous that neither the cold steel of logic nor the white heat of passion found them entirely easy to dispose of. Psychology ought to be grateful to him, for the resulting controversies of the 1920's about Consciousness, Introspection, Instinct and Purpose, together with the proper subject and methods of psychological science, were so profound that when the dust had settled a fresh start in simpler style and freer from past tradition was possible. Much that had been thought before could never be thought again, and stocks of ammunition for unprofitable controversy were a good deal depleted. Watson, however, was no mere professional *enfant terrible*. He was one of a group of workers, chiefly in the United States, who were laying the foundations of comparative and physiological psychology as we know it today. Watson's attempts, armed with Pavlovian bell and candle, to exorcise the ghost in the machine, were sustained by considerable publicity. Very much less in the public eye were the more patient efforts of Lloyd Morgan, E. L. Thorndike, Shepherd Ivory Franz, Karl Lashley, and of Watson himself, to set about investigating animal behaviour and its mechanisms on a solid scientific basis. This work as a whole comprised a number of different elements. Lloyd Morgan contributed the Canon which allowed escape from the anecdotalism of earlier students like Romanes. He also initiated systematic controlled experiment. Thorndike's invention of the Puzzle-Box in 1898 was the beginning of a long series of pieces of special laboratory apparatus, such as the maze, the Lashley jumping stand and the operant conditioning box, which enable performance to be quantitatively assessed. Yerkes, foreseeing a need to move beyond the cats, dogs, hens and rats commonly employed, undertook a major study of the general behaviour and mode of life of primates. When, within the last fifteen years, laboratory workers have increasingly turned their attention to these animals, the background knowledge gained by Yerkes has been invaluable. Another essential feature in the story dates back further. In the first quarter of the nineteenth century Pierre Flourens first practised the surgical removal of parts of the cerebral hemispheres as a means of determining their functions. Throughout the nineteenth century this technique became progressively more refined in the hands of Ferrier, Goltz, Munk and other workers. By the end of the nineteenth century an inconclusive phase had been reached because of the lack of any but qualitative observational methods for assessing the resultant disturbance of function. This was rectified in 1902 by S. I. Franz at the Harvard Physiological Laboratory, who first took the vital step of combining the method of ablation with the quantitative techniques of behaviour measurement

devised by Lloyd Morgan and Thorndike. Franz's findings, which had to do with the functions of the frontal lobes in the cat, could scarcely be regarded as conclusive by present-day standards. He gives no detailed post-mortem account of the extent of the lesions (although he promises one, including microscopical appearances, for later publication). But this paper is a landmark, for in every other respect it is the first representative of a method of discovering the mechanisms underlying behaviour which, gaining momentum over the next half-century, now promises to be as fruitful as any.

Franz made a second outstanding contribution when he encouraged Lashley to widen the scope of this attack so as to cover generally the rôle of the cerebral cortex in intelligent and learned behaviour. 'The whole theory of learning and of intelligence', wrote Lashley some ten years later, 'is in confusion. We know at present nothing of the organic basis of these functions and little enough of either the variety of uniformities or their expression in behaviour.' This is, indeed, too favourable a view of the situation at the outset of his adventure. Seventy years of clinical observation on brain-injured and brain-diseased human subjects and a half-century of animal experiment had afforded a limited number of conclusions, chiefly regarding sensory and motor function. But over these crude landmarks there swirled a mist of sophisticated speculation and controversy. Originally directing his work, Lashley tells us, 'toward the tracing of conditioned reflex arcs through the cortex, as the spinal paths of simple reflexes seems to have been traced through the cord', he cut down his problem to essentials so bare that it might have seemed his findings could be of no general interest at all. But, at long last, a systematic experiment on the cerebral mechanisms of behaviour was carried out. Removal of various amounts of tissue from various parts of the brains of rats, careful post-mortem verification of the lesion, training of the animals in standardized tasks of varying complexity before or after operation were the features of an experiment which gave repeatable results. With these findings, any theory must come to terms, and as Lashley notes, the findings never fitted in to a conditioned reflex scheme. Perhaps because they found this comforting, some people did not hesitate to extend Lashley's tentative conclusions, as expressed in the principles of Mass Action and Equipotentiality, to higher level brains, not excluding man's. Others set about exploiting the basic methodology and extending its application to different species of animal and to a great variety of more special aspects of behaviour. These latter now include not only cognitive but appetitive and affective functions. If today we can say that the experimental approach we owe to Franz and Lashley is perhaps the most powerful at our disposal, this does not mean that it provides a royal road to the solution of every problem even of animal psychology. But from present indications it seems clear that there is still a lot of life in it.

After the First World War direct observational study of instinct and appetite fell on evil days. MacDougall, stirred beyond mere scientific zeal by Watsonian behaviour and unable to accept the particular form of unity which Freudian theory offered, stretched too much the existing knowledge of instinctive behaviour in his efforts to attain a solid theoretical structure. This, together with the increasing scope and success of quantitative experiments on cognitive functions, deflected attention from the problem of what it is that makes animals tick. Pioneers had to be born afresh. Of these, there must have been a number in various countries who were curious and fascinated enough by animals in their natural habitat not only to observe them closely but to try varying systematically that we should now call the 'stimulus parameters'. (In practice, this consisted in such simple measures as substituting billiard balls and wooden cubes for the eggs of a nesting gull.) In this country we ought not to forget the names of Eliot Howard and F. B. Kirkman in this connection. Amateur naturalists both, they pointed the way between them to two methodological principles which, in the hands of Lorenz, Tinbergen and their colleagues, have introduced a fresh and powerful element into the study of behaviour. First, establish by quantitative observation the temporal sequence and topographical pattern of an animal's appetitive activities: second, ascertain by simple but systematic interference with the environment the stimuli which arouse and at every stage control the behavioural sequence. Simple and straightforward as these principles might appear, the adoption of the second must certainly have required notable freedom from anthropomorphic sentiment. It is difficult, watching a nesting goose, not to suppose that

to the bird her eggs are eggs—not just coloured shapes. The decision to find out what range of objects will evoke the same behaviour as the egg is a considerable step forward. There are some branches of human psychology in which almost equally crude approaches might not be unprofitable. The work of the (self-styled) 'Ethologists' has provided a mass of concrete knowledge. Moreover this is in a form which we may expect can be greatly extended and refined by the use of physiological and pharmacological expedients. From the existing data some interesting fresh conceptions have emerged such as the idea of the innate releasing mechanism, of displacement activity and of the distinction between a consummatory act and the satisfaction of a need. These ideas, though incisive, are not rigid and seem capable of considerable modification before they give way to others more precise. At a more theoretical level, the ethologists have formulated interesting schemes for the mechanisms which they suppose to underlie the functions they observe. Not everybody, in particular those wedded to a more strictly behaviouristic approach, approves of them. Here, as in the case of the 'black box' theories evolved by the Hullian school, there is a need for direct check by physiological observation. The ethologists, however, would be the first to agree that theories based on behavioural observation ought to lead toward, rather than away from, the possibility of physiological confirmation. This can less confidently be asserted of the followers of Hull, who seem rather inclined to keep their theories locked up in black boxes with a view to guaranteeing the independent status of psychology against the physiological enemy without. Even among them, however, there have been recent signs of a change of heart.

There must be a great many people who, while they acknowledge the interest and suggestiveness of research upon animals, feel that experimental psychology is burking the issues and evading its responsibilities unless it tackles problems of human behaviour by experiments on human subjects. Personally, I am not wholly in agreement with this view. I believe we are only at the beginning of what we can learn about ourselves by studying animals. Nevertheless it is exciting when new ways of approach to understanding human functions open up in the laboratory and lend themselves to satisfactory experimental treatment. At the present day there are a number such but time forbids mention of more than one. I choose some recent work on speech and language because it exemplifies the way in which practical requirements can evoke attack upon old problems from an entirely fresh angle, and how once such an attack develops on a narrow and minor front it may widen into something of a campaign. When, shortly after its invention by Graham Bell in 1876, the primitive telephone was demonstrated to Lord Rayleigh, he expressed his admiration but doubted whether it would ever be of any practical importance. For a number of years after that the efforts of engineers were devoted to perfecting the faithfulness of speech transmitted down a wire or through the ether. In the meantime Lord Rayleigh had been proved wrong and two new problems presented themselves and became acute in the flurry of war. The first was that of how to economise the channel-space. Copper wires and radio wave-bands are not unlimited. The second arose because existing circumstances and technical resources could and did make several separate communication channels converge upon the same human being. The question of his capacity to sort out and act upon a number of messages arriving simultaneously or in quick succession became literally a vital one. Awareness of the first of these problems led engineers, pre-eminently Claude Shannon of the Bell Telephone Laboratories, to ask what is basically involved in the transmission of information. He concluded that it consists in a decision by the listener, or receiver, as to which of a finite number of possible messages was sent by the speaker, or transmitter. In practice, if less than the whole of a verbal message arrives at the receiving end, we can often make a guess, based on our implicit knowledge of the probabilities of word-sequences, at what the missing words were. This means that for speech to be understood it is not necessary to burden the communication channel with the whole of it. To this extent, language is redundant and telephone lines and radio channels can be economised. In order to study the redundancy of language, Shannon devised means of constructing messages on a statistical basis in which the word-sequence is determined only by the transition probabilities between words, and not by any intention to say anything. Thus a sixth-order approximation to English is obtained by choosing each word only in the light of the preceding five.

Shannon also found a way of estimating the redundancy of specimens of actual English utterance. Such measures provide a new quantitative dimension of language, and this has proved very useful in experiments that have been done on the second problem I mentioned above, that of the mechanism by which we sort out a number of messages arriving simultaneously in our ears and pay attention to one. A good deal of work has recently been devoted to this question by Broadbent, Cherry, Taylor and others. It would seem clear that if we are able to give evidence of attending to one message either by repeating it aloud as it comes or giving its substance later, this must be because other, competing messages are rejected or filtered out. The question is, upon what clue do these filters operate? If we can find this out, we might eventually be able to localise them in the hierarchy of neural functions. A large number of experiments have established that at least part of the filtering is carried out in accordance with differing physical properties of the messages. Thus, if two messages come from spatially separate sources, or are of different pitches like a man's and a woman's voice, this helps to separate them. A filter working on this principle, without reference to any aspect of meaning, could evidently be situated relatively low down in the neural hierarchy. But it is clear that this is not the only filter in operation. If, in accordance with a procedure first used by Cherry, we get a subject to repeat a message that is coming into his right ear while a second one is being delivered to his left, he can do this, and can afterwards give no evidence that he was in any way aware of the content of the rejected message. Indeed, if the language in which the message is spoken is changed from English to, say, German, he simply fails to notice the fact. Certainly, part of the capacity to accept and respond to one message in competition with another has to do with the probability structure of the message. Other things being equal, the more a message incorporates accustomed sequences and a continuing theme, the easier it is to be accurate in repeating it aloud. Miss Taylor had subjects repeat aloud, 'shadow', messages which were in fact only statistical approximations to English of various orders. The competing message in the other ear was normal English. She was able to show that the number of errors made was a very simple function of the order of approximation. But differences at the physical level, and at the level of linguistic structure are certainly not the only factors which operate the filtering system. Everybody knows that at a cocktail party, when one's ears are beset by a most complicated pattern of sound waves, and one is filtering out everything except the voice of the person one is talking to, one's attention can still be caught by hearing the sound of one's own name. This phenomenon has been verified under carefully controlled conditions by Moray and Miss Taylor. It certainly adds fascination to the whole problem, for we may well ask what kind of a system is this which filters out unwanted sounds in accordance with their mere physical characteristics or their mere statistical structure, yet somehow remains on guard at the level of meaning, ready to ring an alarm bell when there may be good reason for turning one's attention elsewhere.

I have not referred to these experiments because of their intrinsic interest but because, as it seems to me, they represent a happy instance in which easily observable facts, lending themselves unexpectedly to quantification can be exploited so as to throw light on very different matters such as attention and meaning. A century ago these would have been thought incapable of any exact enquiry.

It will by now be clear that I have not tried to give any balanced outline of the past hundred years of experiment in psychology. But perhaps even the limited view we have gained may suggest some points about the practice of our craft. If, for brevity, I put these as injunctions, I hope I shall not be misunderstood. First: neither be deceived by the sophistication and respectability of a well-established controversy, nor attempt directly to settle it in the terms in which it is conducted. You will certainly fail, and the terms are probably unreal anyway. Instead, take some question connected with it, however crude and simple, which has not yet been answered and try to answer it. Other questions will follow. Second: try, whenever possible, to be quantitative. Words and ideas without numbers have an almost unlimited power of evading logic. Third: do not invent new descriptive terms and concepts more than is strictly necessary. They are apt to get out of control. Fourth: in pursuing experimental work, try to keep in as close touch with physiological and anatomical factors as possible, not with any immediate hope of

explanation in these terms but because experience shows this to be an effective way of opening up fresh and often unsuspected phenomena for study. Fifth: do not neglect the opportunities offered by a systematic natural history approach, especially when strict experiment seems to come to a dead end.

So much for the centenary. What of the outlook? There is a good deal of talk today about the need for much more research in the human sciences. Many people urge that a great deal more money ought to be spent on these and some seem to assume that, if it were, commensurate results would follow. Psychology is one, perhaps the most basic, of the human sciences. Money is the root of all evil, but so far as psychology is concerned I would think it is true to say that if all the research which could now profitably be done were in progress a great deal more money would have to be spent than is being spent at present. Many fresh fields are opening up in which, especially in collaboration with physiologists, zoologists, pharmacologists, geneticists and engineers, the experimental psychologist can work with every prospect of being led to basic issues. A vast development of psychology like that in the United States would not, perhaps, recommend itself to our temperament. But it must be said that on any count which gives due weight not only to quantity but quality, this country is some way behind America in behavioural research. The prospects for experimental psychology here may nevertheless be good. To make the best of them, however, may require some re-orientation on the part of all of us who pursue the subject. The future will turn not on individual sallies of brilliance, nor on the worthy pursuit of the solid and impeccable. What will matter is the quality of the young people who are drawn into the subject. Once attracted, they must at first get their encouragement and the confirmation of their enthusiasm from what goes on in teaching and research laboratories. If not anybody's guess, at any rate it is a matter of opinion as to what will most contribute to a favourable atmosphere. May I be allowed to finish by giving my view—briefly but frankly?

First, I will affirm a belief, which I think many people share, that the growth of psychology will be within the framework of the biological sciences. This means striving to regard human behaviour as objectively as the biologist looks at the behaviour of animals. It cannot be too strongly asserted that this implies no lack of sympathy, respect or love. Nor does it mean that psychology is for ever constrained within any existing framework of biological ideas. It is far more likely that psychology will be instrumental in extending the range of fundamental biological conceptions. Secondly, so it seems to me, we have no need to wax protective about our subject—to expend effort on advertising the special and individual character of its approaches, methods, concepts, and theories. There is no point in emphasising the 'psychologist's' view of human beings and their behaviour. For better or worse, experimental psychology is an established activity, whether it is carried out at a department which has Psychology, Zoology, Medicine or Engineering on the door. Its future will lie in the hands of those who pursue it, not in the maintenance of frontiers. I say this because I sometimes feel that we who call ourselves psychologists stand in some danger of worshipping the Idols of the Parish.

But there are less distasteful thoughts. If the work of the past century has done nothing else it has recast some problems, and uncovered others in forms susceptible to direct systematic attack. At the same time appropriate technical and instrumental resources have emerged. The usefulness and the limitations of work upon animals have become much more clearly appreciated. We no longer suppose that even careful controlled experiments on the abilities of rats to run mazes after removal of part of their brains is likely to make any particular contribution to the general problem of our own cerebral processes. But specific possibilities, such, for instance, that the maintenance of conscious vigilance is effected by some kind of closed circuit system, into which stimuli from the outside world are fed via two separate channels, can be looked into with a precision undreamed of twenty years ago.

Another unquestionably favourable feature is the increasing scale and freedom of collaboration between psychologists and those who work in a great variety of other fields. That this takes place is partly due to another factor which I personally would rank as the most important element of all. This is the growing number of young people coming into the subject whose ability would distinguish them in many other fields. Do these gain, during their time as undergraduates and

postgraduates, the knowledge and experience of problems and methods which will stand them in best stead later, whether in research or in other psychological connections? Nobody can fail to note the progress made by university departments of psychology in the past twenty years. But psychology, at any rate for the present, cannot stand by itself. It demands, just as do other biological sciences, connection with and support from other disciplines. A number of our students, some among the best, have missed the opportunity of getting practical grounding in any of the basic scientific disciplines. To an astonishing extent, the better students grasp from reading and lectures the salient points, say, of contemporary neurophysiology or the leading ideas of communication theory. But in doing this they sometimes remain mindful only of the bearing of these subjects upon the psychology which is their chief concern. Sometimes the material is used merely for regurgitation in examinations, or for self-gratifying speculation. Might it not be better to curtail a little the psychological bill of fare and give our students an opportunity of learning the elements of neighbouring scientific fields from teachers, and alongside other students, whose chief concern and enthusiasm lie in them?

Experimental psychology does not lack worthwhile tasks, or the means of carrying them out. The next hundred years promises exciting progress. What the impact on people generally may be it is difficult to foresee. The great majority of human beings give little evidence of really wanting to understand themselves. Or is it just that, in Spinoza's words, 'every excellent thing is as difficult as it is rare'?

THE PERIPHERAL MECHANISM OF HEARING: THE RESPONSE OF THE EAR TO NORMAL AND TO INTENSE SOUND*

By Prof. W. BURNS *Dept. of Physiology, Charing Cross Hospital Medical School*

The central nervous system of animals performs its controlling function in virtue of the flow of information which it continually receives. In the higher animals the quantity and variety of information is very large, and the total effect is to give a constant picture of the external environment of the animal, and of the chemical and physical conditions existing inside the animal's own body. The information from the exterior includes that from the eye, the ear, the nose and the skin, and in different kinds of animal, different emphasis is placed on these sources of information. In man, vision is perhaps the most important. Hearing, however, has a special place in the human sensory equipment, since it is the receptor mechanism for speech and the evolution of language has been inseparably associated with the development of human culture and intellectual achievement.

The study of the ear and of hearing presents physiological problems not only of great intrinsic interest, but is particularly appropriate to the times in which we live. In the first place, the peripheral mechanism of hearing, in which sound in air is transformed into nerve impulses, is a mechanism of astonishing sensitivity, which will, nevertheless, withstand surprising levels of sound energy without damage. It shows a complication and subtlety of structure and function, which has absorbed the thoughts, efforts and skill of physicists, physiologists, psychologists and otologists for many years, and our understanding of the mechanisms is still far from complete. Further, at the present time, new auditory problems have been introduced by the widespread occurrence of noise at levels previously unequalled in normal experience of either natural or man-made sound. Such sound may damage hearing. One of the obvious sources of these exceptional sound levels is the turbo-jet engine, now being introduced into commercial airline operations. Very few people indeed are ever sufficiently exposed to the noise of these engines to be in danger of damage to their hearing, and if they are, they can be protected. However, the presence of jet-engined aircraft has served to bring noise, as a source of annoyance and disturbance, to the attention of the public in a way which less dramatic noises have failed to do. This is despite the fact that the latter, in a fairly intense form, have existed, for example in heavy industry, for more than one hundred years, and in the form of gunfire, for much longer.

This would appear, therefore, to be an appropriate time to consider certain aspects of the physiology of hearing, together with some reference to the effects of intense sound on the ear. A comprehensive review of the physiology of hearing is impossible in the present context; therefore only the peripheral part of the hearing mechanism will be considered. This will include the events, starting from sound vibration in air, which culminate in the production of nerve impulses in the auditory nerve. The subsequent stages will be ignored, but not on grounds of unimportance. On the contrary, they involve complicated neural events, and the final distribution of the auditory pathways still presents a picture of confusing complexity. In the following account, numerous sources have been drawn upon, including the St. Louis school of auditory physiology under Dr. H. Davis; the work

* Address delivered to Section I (Physiology and Biochemistry) on September 2, 1960, at the Cardiff Meeting of the British Association.

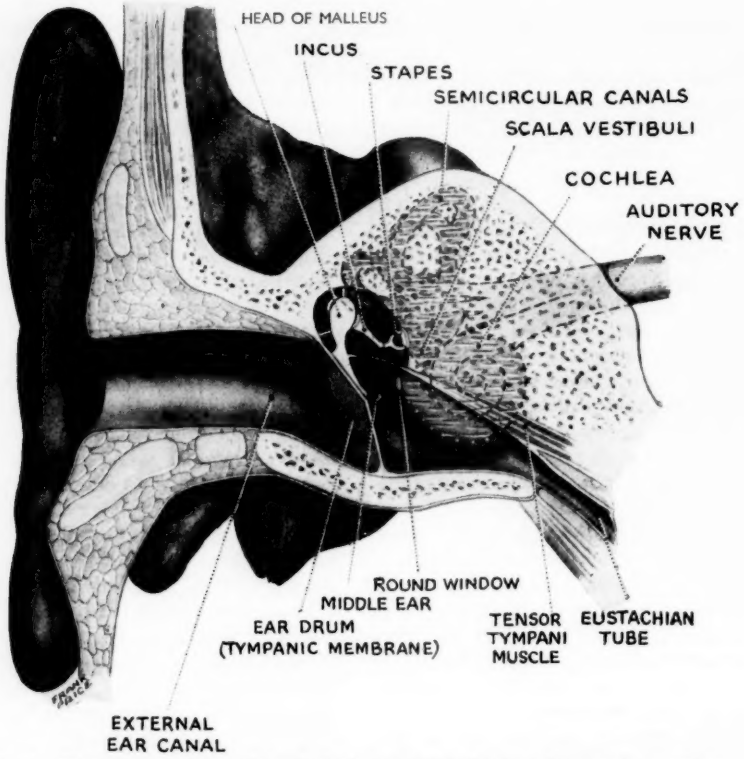

Fig. 1.—Diagrammatic section through the human external ear canal, tympanic membrane and middle ear cavity. The cavities within the bone, including those of the cochlea and semicircular canals, are indicated in outline.

of Békésy; of Wever, and of many others. A short list of references is included.

1. THE TRANSFORMATION OF SOUND ENERGY INTO NERVE IMPULSES

The physiological aspects which are considered here may be divided into a number of sections governed by considerations of structure and function. The general structure of the hearing mechanism is shown in Fig. 1. The events which must be described start with the occurrence of sound pressure fluctuations in air; these in turn set into vibration the ear drum or tympanic membrane, which in turn actuates a lever system of three very small bones, the auditory ossicles, situated in the air filled cavity of the middle ear. The ossicles are grouped in such a way that the vibrations of the tympanic membrane drive the ossicle known as the malleus, which drives the second ossicle, the incus, and finally the third ossicle, the stapes, sets up vibrations in fluid and certain structures contained in the cavity in the temporal bone known as the cochlea of the internal ear. In the cochlea is the essential end-organ of hearing, in which mechanical vibrations are transposed into nerve impulses in the fibres of the auditory nerve. The information travels in this coded manner to the appropriate parts of the brain, there to be perceived as a sensation, probably stored as a memory, capable of arousing pleasure or annoyance, as well as having other secondary effects. We may now consider

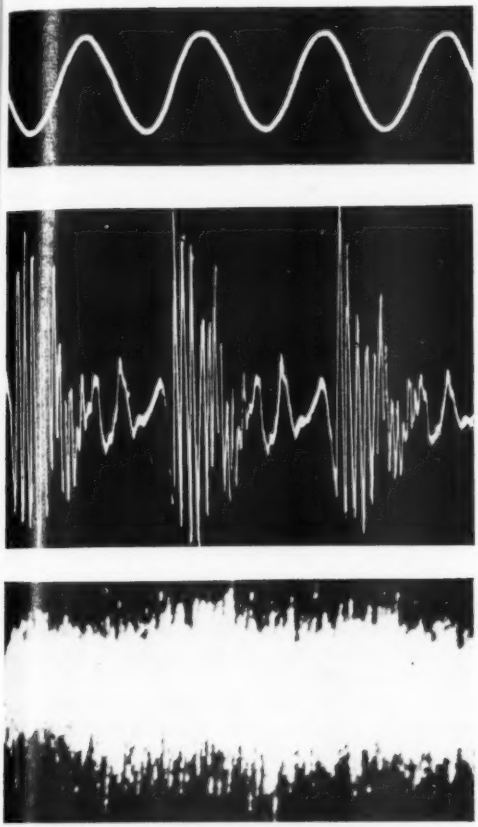

Fig. 2.—Waveforms of sound pressure (vertical axis) as a function of time (horizontal axis): (*a*) a pure tone (tuning fork); (*b*) a musical note (motor horn); (*c*) unpitched noise (electronically produced wide band noise). (*Reproduced in part by courtesy of Dr. T. S. Littler, from* Annals of Occupational Hygiene, **1**, 11, 1958.)

these different aspects of the hearing mechanism in more detail, but first, we should briefly define the nature of the stimulus.

Sound

Sound consists of energy in vibratory form, which may travel in solids, liquids or gases; the ear is specialised for the reception of airborne sound. In air, sound can be regarded fundamentally as an oscillation of pressure above and below the prevailing atmospheric pressure. The magnitude of these oscillations may be expressed as the sound pressure, and the energy or intensity of a sound field is usually expressed in terms of the square of the sound pressure. The ear is a device which reacts to sound pressure, and its magnitude, to a certain extent, determines the loudness of the sound. Some sounds consist of only one note, or pure tone, such as the sound of a vibrating tuning fork. In this case the motion of the blades, and the consequent changes of air pressure will be of the type known as simple harmonic.

Such a sound expressed visually as a curve of the sound pressure at each instant as a function of time, is shown in Fig 2*a*. The pressure is seen to rise and fall in an orderly manner and the individual cycles occupy the interval of time necessary, for example, for the pressure to rise above, fall below, and again attain the atmospheric value. The number of times the cycle is repeated, say in an interval of one second, is expressed as the frequency of the tone in cycles per second.

However, most sounds contain not one, but many different frequencies. The musical sounds, such as a vowel sound of the human voice, the sound of a musical instrument or the sound of rotating machinery, show a series of complex but virtually identical variations consisting of many different frequencies. These complex pressure waves have nevertheless a regular frequency of recurrence called the fundamental frequency (Fig. 2*b*). The safety valve of a steam locomotive, however, when discharging steam, emits sound containing a wide range of frequencies, called, on analogy with white light, white noise, but possesses no regular cycle of recurrence of the pressure variations (Fig 2*c*); a hissing or roaring sound results. In a free and unobstructed mass of air, sound spreads in waves of pressure, which travel at a velocity dependent on the temperature of the air. At ground level in temperate conditions the velocity is of the order of 1100 feet per second.

Sensitivity of the ear

The ear is extremely sensitive to sound pressure changes. The normal ear of young people is most sensitive to sound frequencies in the region of 1000–3000 cycles per second and less sensitive below and above this range. The normal atmospheric pressure is about 1 million dynes per square

centimetre, and the ear can detect pressure changes of the order of one thousandth of one dyne per square centimetre; that is one thousand millionth of an atmospheric pressure. At the level of pressure where sounds begin to cause pain in the ear the magnitude is about one thousandth of an atmosphere, so that the effective range of pressure from the least perceptible to the short-term level of tolerance is about one million times. The achievement of this range, as well as the degree of sensitivity, is a phenomenal performance. At the threshold of hearing, it has been calculated that the actual receptor cells are being moved through a distance of about the diameter of a hydrogen atom and any appreciable increase in sensitivity would be impracticable, since it would detect physical background noise due to Brownian movement.

We must therefore consider how the ear achieves such sensitivity, such dynamic range, and how it can differentiate between various frequencies and sound pressures, and how the information is coded in the auditory nerve. The details of the excitation process which converts movement of structures into nerve impulses are of fundamental interest.

We may now look at the initial stages, the conduction of pressure changes across the middle ear.

External ear canal and middle ear

The purpose of these structures is to conduct sound vibrations into the inner ear, and to deliver them to its contained fluid in a suitable form, in terms of force and amplitude. The middle ear also has a protective function, which partly guards the internal ear structures from excessive sound pressure fluctuations.

Starting at the visible external ear, which in man is less important than the large movable external ears of many animals, we may note that the position on each side of the head, with the mass of the head in between, makes possible the directional identification of sound by differences of intensity, and of the time at which the sound arrives at each ear. The sound is conducted down the external ear canal, which is just over one inch long, and impinges on the tympanic membrane or ear drum, a stiff conical diaphragm with flexible margins. The tympanic membrane is made to vibrate in synchrony with the sound pressure changes, with an amplitude dependent on their magnitude (Fig. 3). The tympanic membrane is the boundary between the external ear canal and the little cavity, filled with air, known as the middle ear. This communicates with the back of the nose-throat cavity through the Eustachian tube, which is normally closed, but opens on swallowing to preserve equal air pressure on the two sides of the tympanic membrane. The middle ear also communicates with air filled cells in the mastoid bone, behind the external ear, and infection may involve both middle ear and mastoid cells. The sound vibrations are imparted to the chain of ossicles in a manner dictated by their linkages and supports. The malleus and incus are normally locked together; they oscillate through a small arc about an axis which is approximately a tangent to the upper rim of the tympanic membrane, driven by the handle of the malleus, which is attached to the inner surface of the membrane. The incus in turn drives the stapes, which forms a small partition, intervening between the air of the middle ear cavity and the fluid filled spaces of the internal ear and filling the orifice in the bone known as the oval window. The oscillations of the stapes are made possible by a flexible rim, which seals the fluid in the internal ear, and the little bone rocks in the oval window, like a bellows hinged at the rear edge of the window. The inner ear fluid is thus also made to vibrate, and the necessary elasticity is given to the closed fluid-filled cavity by a second aperture in the cavity, the round window, which is sealed by an elastic membrane. The area of the stapes in the oval window is only one twentieth that of the tympanic membrane, and the vibrational displacements of tympanic membrane and stapes footplate as well as the forces at each, are about equal. The pressure per unit area on the fluid of the internal ear is thus much greater than that on the tympanic membrane, and the transfer of energy to the fluid of the internal ear is very efficient. This is probably the main function of the middle ear. Among other functional aspects is that of protection. Two small muscles (tensor tympani and stapedius) are attached to the malleus and the stapes and when intense sounds occur, or objects touch the external ear canal, these muscles contract, tending to pull the stapes and the tympanic membrane towards the middle ear cavity, and putting increased resistance to movement into the ossicular chain. Sounds, particularly of low frequency, are then transmitted with increased loss, so partially protecting the internal ear from damage.

Internal ear

The internal ear, as we have seen, consists of a system of canals filled with fluid, deep in the bone of the skull, and thus protected mechanically and acoustically (Figs. 1 and 3). The fluid which is set into vibration directly by the vibrations of the stapes is contained in the coiled tube called the cochlea from its resemblance to a snail's shell. The bony cavity of the cochlea, starting at the base, ascends for about 2½ turns to the apex in the human ear. This coiled tube is split longitudinally by a partition which ends below the apex, so forming a separate canal on each side of the partition, with a communication known as the helicotrema at the apex. These two canals are filled with a fluid, perilymph, similar to the cerebrospinal fluid surrounding the brain and spinal cord. At the base of the canals, in the scala vestibuli, is the oval window, with the footplate of the stapes in it, while at the base of the other canal, the scala tympani, is the round window and its elastic membrane (Fig. 3). Vibration of the stapes will set the fluid into motion and with it, the round window and the partition between the scalae. This partition is all important, since it carries the end organs which translate movements of the partition into patterns of nerve impulses in the auditory nerve. The partition is actually another tube, the scala media, containing a different fluid, the endolymph, which appears to be secreted by cells of the stria vascularis, covering the outer wall (Fig. 4). The other boundaries of the cochlear partition separating the scala media from the scala vestibuli are two long ribbon-like membranes; on the side towards the scala vestibuli is Reissner's membrane, a thin flexible wall, while on the side towards scala tympani is the basilar membrane, on which rests the receptor organ, the organ of Corti. The basilar membrane, about 35 mm.

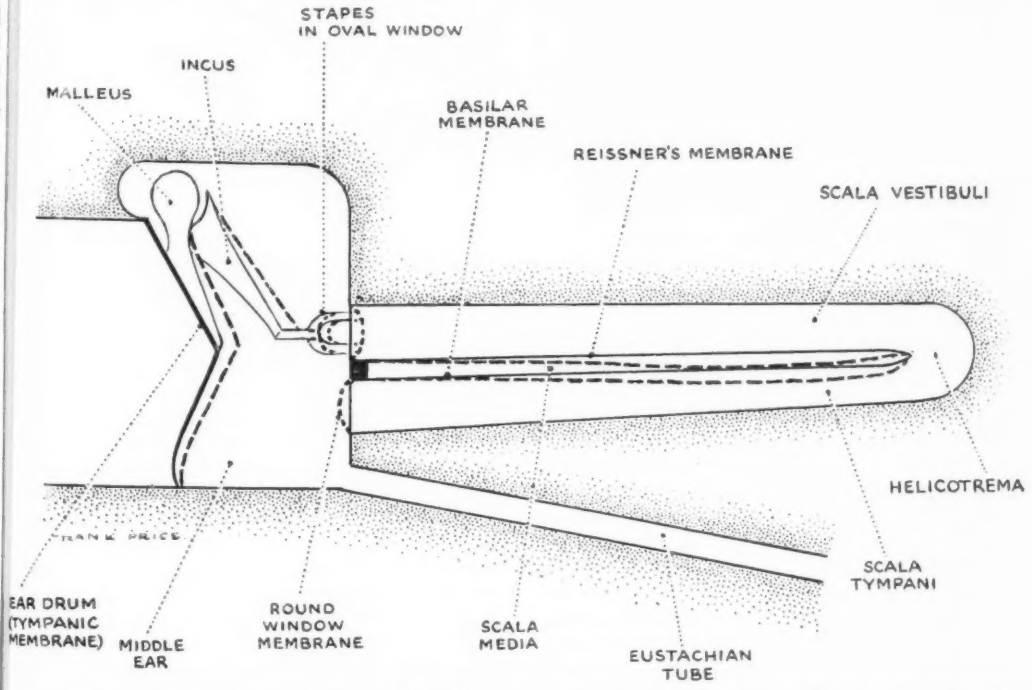

Fig. 3.—Diagram of the main components of the hearing mechanism. The cochlea is shown straight for clarity. The solid outlines show the movable parts at rest; the interrupted outlines show the structures deflected by the rising (i.e. above atmospheric pressure) phase of a low frequency sound wave.

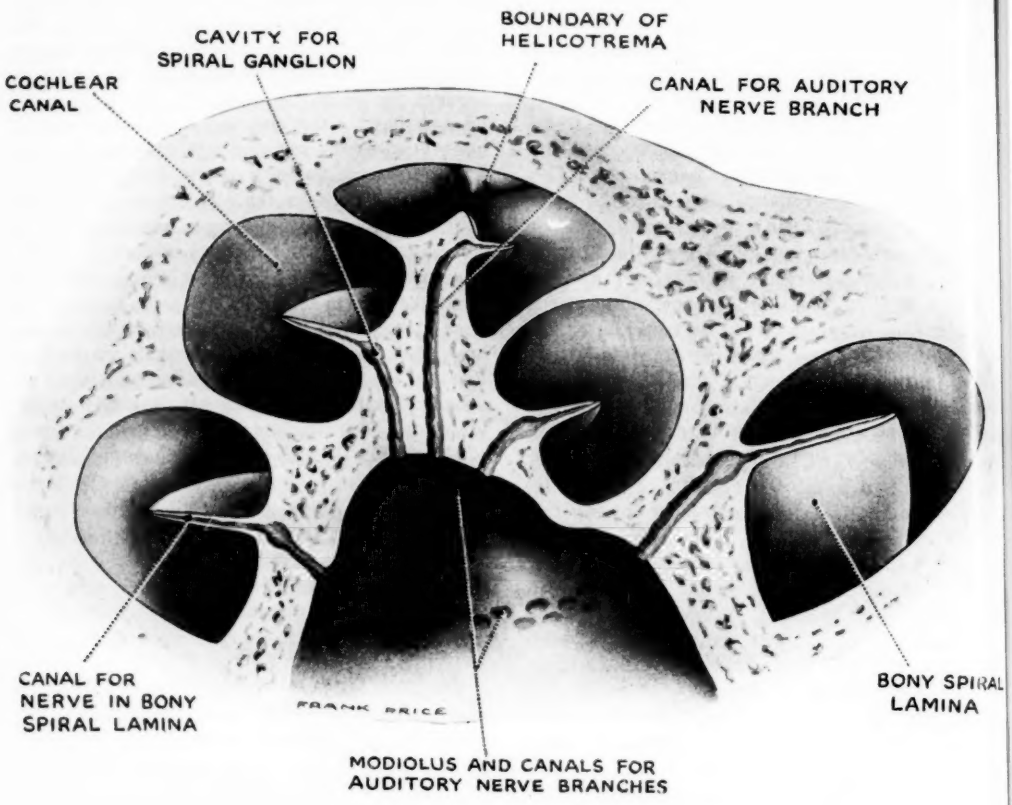
BOUNDARY OF HELICOTREMA
CAVITY FOR SPIRAL GANGLION
COCHLEAR CANAL
CANAL FOR AUDITORY NERVE BRANCH
CANAL FOR NERVE IN BONY SPIRAL LAMINA
BONY SPIRAL LAMINA
MODIOLUS AND CANALS FOR AUDITORY NERVE BRANCHES

Fig. 4 (*a*).— Schematic section through the human cochlea, showing the bony cavity only.

long from base to apex is narrow (0·04 mm) at the base and broad (0·5 mm.) at the apex, with a gradual change in width. The cochlear partition and particularly the basilar membrane, which are structures possessing elasticity, have a graded variation of stiffness and mass along their length, so that the different parts of the membrane resonate at different frequencies, high stiffness, low mass and a maximum response to vibrations of high frequency being located at the basal end. The whole partition possesses other physical characteristics which also determine its motion in response to sound vibrations conveyed through the tympanic membrane and ossicles.

Organ of Corti

This is a complicated system of cells and associated tissues extending along the entire length of the cochlear partition. Except for the different width of the basilar membrane and consequent gradual variations of dimensions along the length of the organ of Corti, the structure remains unchanged in general architecture throughout the whole of the cochlear partition. Basically it consists of three or four rows of external hair cells (Fig. 5) with certain supporting cells and structures, which rest on the basilar membrane on the peripheral side. The hair cells are elongated, with the small bristle-like processes which give the cells their

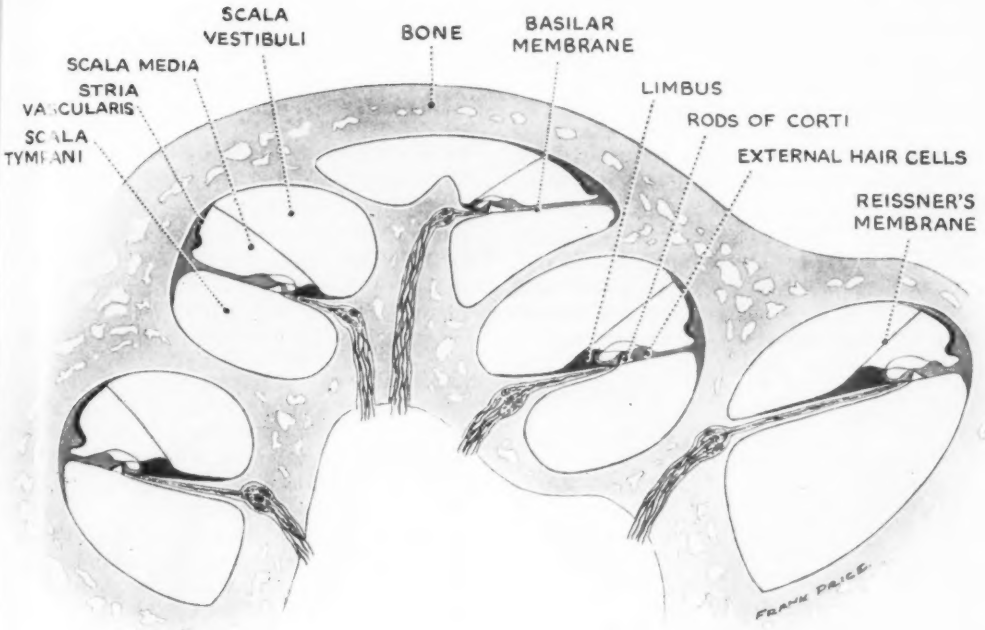

Fig. 4 (*b*).—Schematic section as in Fig. 4 (a) but with the non-bony tissues added. The scala media, formed by the basilar and Reissner's membranes, and containing the organ of Corti, ends just below the apex of the cochlea. Scala tympani and scala vestibuli communicate here at the helicotrema. (*a*) *is redrawn by courtesy of Prof. W. J. Hamilton, from* Textbook of Human Anatomy, *McMillan*, 1958.)

name projecting upward into the scala media, and anchored by their tips to the underside of the tectorial membrane, which runs as a spiral shelf over the organ of Corti. Separated from the external hair cell complex by a row of A-shaped struts, the rods of Corti, are the internal hair cells, with some supporting cells. The internal hair cells are on the side nearer the axis of the cochlea and are arranged as a single row, again for the entire length of the cochlea, and they lie virtually over the inner margin of the basilar membrane. Their hair processes are also embedded in the under side of the tectorial membrane. The upper extremity of the hair cells is anchored by a thin strong sheet, the reticular lamina, with openings into which the ends of the hair cells fit. The other ends of the cells rest in sockets formed by the cells known as Dieter's cells. The outer hair cells are in diameter about the same as a red blood corpuscle (i.e. about 8 microns) and number about 20,000. The inner hair cells are somewhat larger in diameter and there are about 3500, extending in a single row. The shape of the long shelf-like tectorial membrane, as seen in section (Fig. 5) is significant. It becomes thin and flexible at the inner edge, and in fact hinges about this point, as may be seen when we consider the mechanical movements of the organ of Corti. The nerve fibres which make up the auditory nerves start at the hair cells. They are applied to the exterior of the hair cells, the cell bodies of these nerve fibres being grouped together as the spiral ganglion as they converge towards the modiolus (Fig. 4) or

central axial cavity of the cochlea, on their way to the brain. An important feature of the structure significant from a functional viewpoint is that only the ends of the hair cells carrying the hair processes are in contact with endolymph; the space between the rods of Corti, known as the tunnel of Corti, contains perilymph from scala tympani. The nerve fibres are distributed to the hair cells in a complex manner; the external hair cells share nerve fibres, one fibre supplying a number of hair cells, and each hair cell deriving its supply from a number of fibres. The inner hair cells, on the contrary are supplied by fibres which only innervate one to three hair cells. There is, in addition to these fibres which carry the information to the nervous system, another category of fibres which carry impulses outwards, capable of modifying the sensitivity of the ear to sound. Their function is not clear.

The significance of these complicated structures in generating nerve impulses from mechanical movement we may now examine.

The cochlea as the end-organ of hearing

This aspect of cochlear function may be examined from two viewpoints. The first concerns the nature of the movements of the cochlear structures, by which the cochlea is able to act as a frequency analyser of sound, and so to contribute to the appreciation of its pitch and intensity; the second viewpoint concerns the means whereby the move-

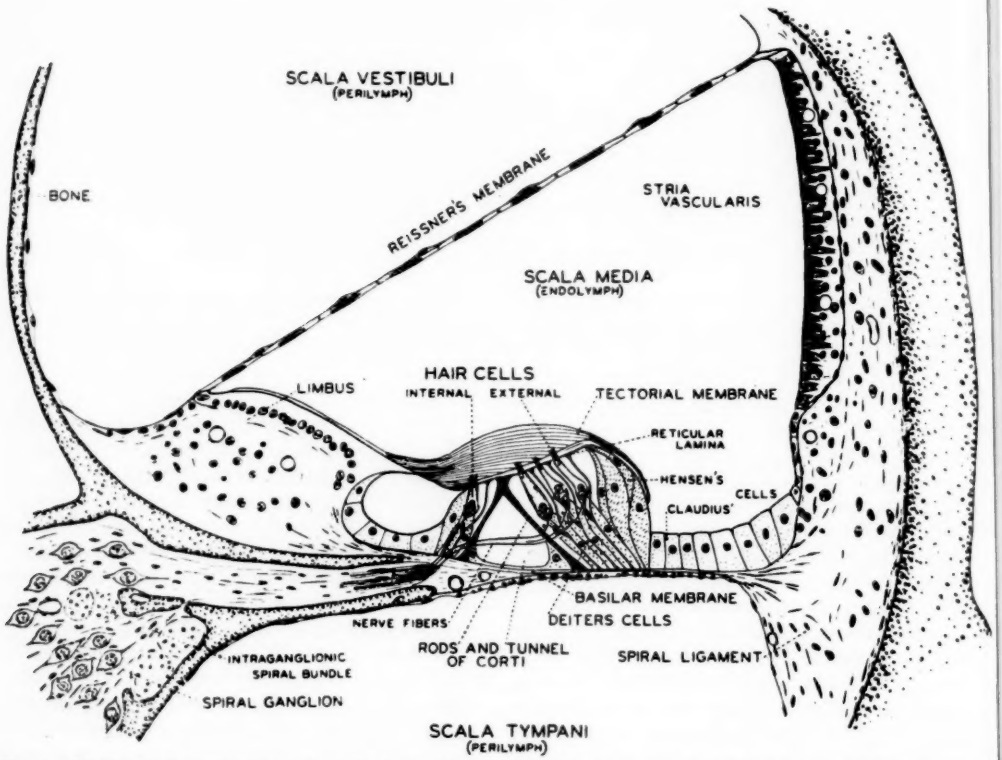

Fig. 5.—Diagrammatic cross-section through the cochlear partition in the second turn of the guinea-pig cochlea. Scala vestibuli and scala tympani are incompletely shown. (*Reproduced by courtesy of Dr. Hallowell Davis and co-authors, from* J. Acoust. Soc. Amer., **25**, 1180, 1953.)

ments are translated into impulses in the auditory nerve.

Movements of the cochlear structures

Let us assume that, due to a sound wave, the tympanic membrane is pushed inwards towards the middle ear, so actuating the chain of ossicles, and causing the footplate of the stapes to move inwards into the oval window. The resultant rise in pressure of the fluid in scala vestibuli travels as a wave along the cochlea, but the transmission of the wave is so rapid that the pressure rise can be regarded as almost simultaneous at all points in the scala vestibuli. The result of this pressure rise on the contained structures of the cochlea is complicated, and determined by the physical properties of the cochlear partition. The latter term, as we have seen, implies all the structures intervening between scala vestibuli and scala tympani, and includes the basilar membrane, organ of Corti, tectorial membrane, the fluid (endolymph) in scala media, and Reissner's membrane. As a permissible simplification, these structures can be considered to move together in response to the pressure changes in scala vestibuli, but the exact nature of the movements depends on the fact that the partition has different values of stiffness and mass, along its length, and that mechanical coupling between adjacent areas and damping are also present. Some of these physical properties are particularly dictated by the characteristics of the basilar membrane. As has been noted, stiffness is maximal and mass minimal at the basal end. In such a system, a rise of pressure in scala vestibuli will set up a travelling wave of transverse displacement (although some longitudinal displacements also occur) in the cochlear partition. This wave, as shown by Békésy, starts at the basal end of the partition, and travels upwards towards the apex, increasing in amplitude as it goes. At a position which is different for different frequencies, the wave reaches a maximum amplitude, and then suddenly diminishes in velocity and amplitude and is soon extinguished. With sounds of appreciable duration, trains of waves of displacement will thus pass up the cochlea from base to apex, while the round window membrane vibrates in the opposite phase to the vibrations of the oval window, so contributing the essential elasticity to an otherwise almost rigid fluid filled cavity. As a result of this complex behaviour of the cochlear partition, it can act as a frequency analyser for sound. High-frequency sounds will excite trains of waves which will reach their maximum amplitude at the basal part of the partition, and thereafter become extinguished. The velocity of travel here is rapid, so much so that the cochlear partition can be regarded as vibrating almost as one unit in the basal turn. For somewhat lower frequencies the wave motion becomes more obvious and travelling waves ascend towards the more apical regions, before suddenly diminishing and dying away; while for the lowest frequencies the waves involve the entire length of the cochlea and reach their maximum in the apical regions. In summary, the movement of the partition is

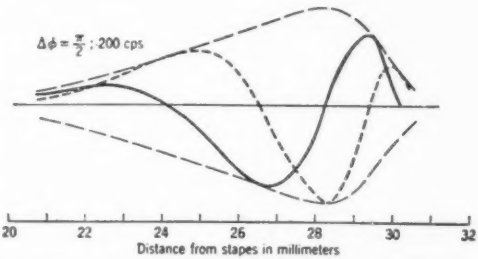

Fig. 6.—The travelling wave on the cochlear partition as described by von Békésy. The vertical scale shows relative amplitudes at different positions along the cochlear partition, measured from the stapes, towards the apex of the cochlea. The solid line shows the longitudinal waveform, in response to a tone of 200 cycles per second, at a given instant. The dotted line shows the same wave one-quarter of a cycle (1/800 sec.) later. The broken outline shows the amplitudes as the wave travels along the partition. Note how the wave reaches a maximum amplitude and dies away. (*Reproduced by courtesy of Dr. G. von Békésy, from* J. Acoust. Soc. Amer., **19**, 452, 1947.)

almost simultaneous at the basal regions, but higher up takes the form of travelling waves, with different velocities at different parts of the cochlea and for different sound frequencies, but reaching a maximum amplitude at a specific position along the cochlea, dictated by frequency in an orderly manner, highest frequencies nearest the base, lowest nearest the apex. Following the peak is a rapid reduction of amplitude and velocity and therefore wavelength, followed by extinction. At the maximum amplitude the wave lags about one cycle behind the pressure wave in the fluid of scala vestibuli, while more

apical to this point the diminution in velocity causes the wave to lag even more behind the pressure wave (Fig. 6).

Generation of nerve impulses

The cochlea belongs to the category of mechanoreceptors, in which the stimulus is mechanical displacement. We must now consider how the movements of the cochlear partition are translated into nerve impulses, with particular regard to the means used to attain the remarkable sensitivity, and the type of coding employed in the auditory nerve to carry the information on frequency and intensity of the sound stimulus.

The action of the organ of Corti in initiating nerve impulses in response to mechanical displacement of the cochlear structures is achieved, we believe, by the use of electrical potentials which stimulate the nerve endings, so producing nerve impulses as a result of mechanical movement of the cochlear partition. To describe these phenomena we must look in more detail at the electrical potentials associated with the cochlear structures. In general, biological activity is associated with the production of measurable potential differences in certain parts of tissues with respect to other parts, attributable to particular distributions of ions. These potential differences may be fairly stable and permanent, and in this category we find that in general the interior of living cells is negative to the exterior of the cells. In certain situations, sudden and dramatic re-groupings of ions across membranes may give characteristic electrical changes, often very rapid and transient, such as those accompanying the passage of nerve impulses along nerve fibres, or the contractile changes in active muscle.

Endocochlear potential

When the cells of the organ of Corti are explored with microelectrodes we find the usual interior negativity in, for instance, the hair cells. In addition, there is a unique situation in the case of the endolymph in scale media, which is at a potential of nearly a tenth of a volt positive to the perilymph in scala vestibuli and scala tympani, and remains so if no sound stimulation occurs. This potential difference is actually about +80 millivolts (mV) with respect to the scala tympani and other extracellular situations in the body, and the potential difference with respect to the interior of the hair cells, which are themselves internally negative, is still greater. The potential difference across the hair-bearing ends of the hair cells is thus some 150 mV. This electrical positivity of scala media is called the endocochlear potential and it is maintained by the metabolic activity of the cells of the long ribbon of tissue on the inside of the outer wall of the scala media, known as the stria vascularis. This endocochlear potential is acutely dependent on adequate blood oxygen and largely fails in its absence. We do not yet know the physiological basis of this potential.

If the relative positions of the structures in the cochlear partition are altered by 'upward' and 'downward' movement (i.e. towards scala vestibuli or scala tympani respectively) the value of the

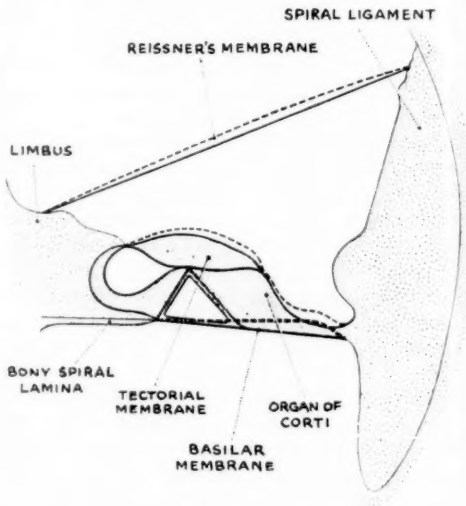

Fig. 7.—Diagrammatic cross section of the cochlear partition to show the transverse displacements, according to von Békésy. Movement of the cochlear partition towards scala vestibuli causes pivoting of the tectorial and basilar membranes about their left hand edges as viewed on the diagram. Because of the different locations of these pivots, a shearing movement occurs between the tectorial membrane and the upper surface of the organ of Corti. (See Fig. 8.) (*Redrawn by courtesy of Dr. H. Davis, from* Physiological triggers and discontinuous rate processes, (section entitled 'Initiation of nerve impulses in cochlea and other mechano-receptors') edited by T. H. Bullock, Washington: American Physiological Society, 1957.)

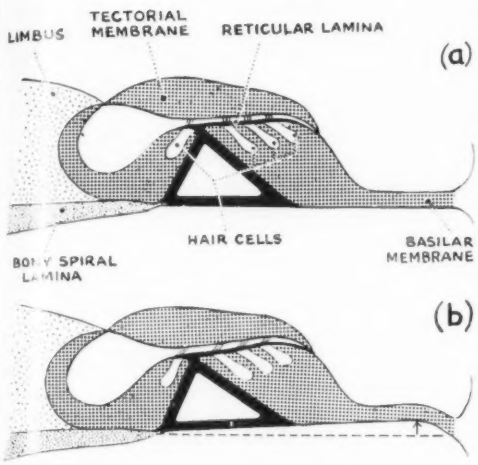

Fig. 8 (*a*) and (*b*).—Diagram of the organ of Corti to illustrate how vertical displacements of the cochlear partition produce shearing forces between the reticular lamina and tectorial membrane: (*a*) structures at rest, hairs of hairs cells undeflected; (*b*) structures displaced upwards, hairs pulled to the right of the diagram. (*Redrawn by courtesy of Dr. H. Davis, from* Physiological triggers and discontinuous rate processes, (section entitled 'Initiation of nerve impulses in cochlea and other mechano-receptors') edited by T. H. Bullock, Washington: American Physiological Society, 1957.)

endocochlear potential alters. If the cochlear partition is moved towards scala vestibuli by pressure on the basilar membrane or an outward movement of the stapes (and so also of the tympanic membrane) the endocochlear potential is diminished. The opposite movement of the organ of Corti, i.e. downward movement of the basilar membrane and organ of Corti, can be caused by raising the pressure in scala media or an inward movement of the stapes, will increase the endocochlear potential. Changes are not influenced by movement of Reissners' membrane, but can be evoked by isolated movement of the tectorial membrane by a micromanipulator. The essential movement is relative displacement of the hair cells and tectorial membrane, so that the hairs are bent one way or the other. Bending the hairs outwards, i.e. away from the axis of the cochlea, reduces the cochlear potential, and vice versa. Because of the different pivoting axes of the basilar and tectorial membrane, any simultaneous 'rise' (or 'fall') of these two membranes, causes a shearing or wiping (Figs. 7 and 8) action across the free ends of the hair cells by the tectorial membrane, so bending the hairs in a radial plane with respect to the cochlea. The maintained changes in the endo-cochlear potential are a consequence of the bending of the hairs due to this displacement of the cochlear structures. The endocochlear potential will remain at a fixed value as long as the mechanical stimulus of modified position of the structures is maintained. We do not yet know for certain whether the endocochlear potential is modified by specific electrical potentials generated by the hair cells in response to position of the hairs; or whether there is a modulated leakage of the current from the positive source in scala media, to scala tympani, dependent on a variation in resistance of the hair cells, determined by the position of the hairs. Such a current leakage must occur, and the hypothesis of modulated leakage is attractive. Both effects may occur.

Cochlear microphonic

From the endocochlear potential and its variations it is but a short step to the consideration of the alternating potentials, evoked by sound stimulation, known as the cochlear microphonic potentials. These changes can be recorded optimally by very small wire electrodes inserted into scala media and scala tympani, but also from a wide variety of other sites, including scala tympani and scala vestibuli. This technique has been evolved at the Central Institute for the Deaf at St. Louis, under Hallowell Davis, and the ear of the anaesthetised guinea-pig is particularly suitable. A diagram of the position of the electrodes and the records, both of cochlear microphonic and action potential in the auditory nerve is shown in Fig. 9. The movements of the cochlear partition, in response to sound stimulation, set up what must be, in effect, fluctuations in the endocochlear potential and which we recognise as the cochlear microphonic. Thus a regular oscillation of the cochlear partition, in response, for example, to a pure tone applied to the ear as airborne sound, would produce an electrical transcript of the sound pressure vibrations, in the form of a sinusoidal cochlear microphonic which faithfully reproduces the sound pressure fluctuations. The potential changes may be picked up by electrodes in the anaesthetised guinea-pig ear in

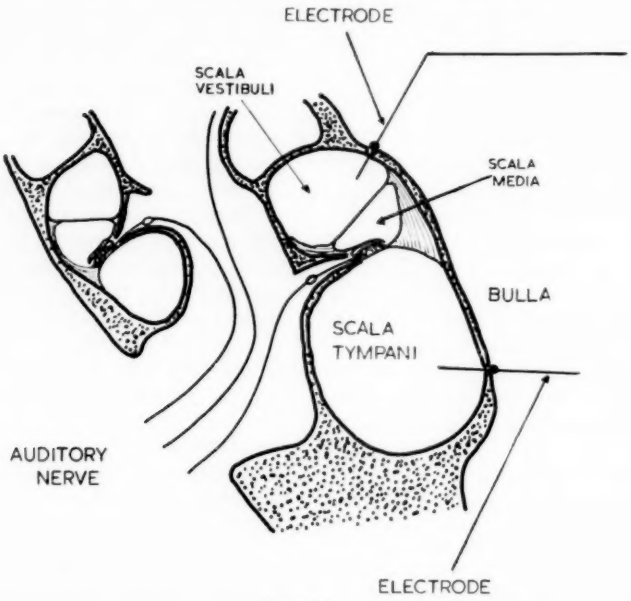

Fig. 9(*a*)

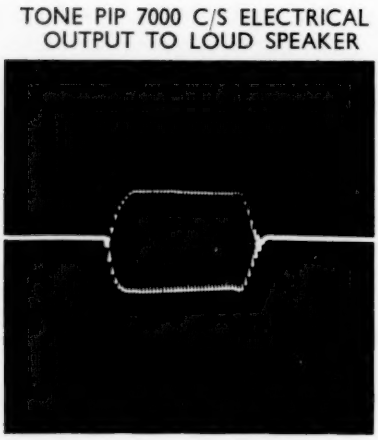

Fig. 9 (*b*)

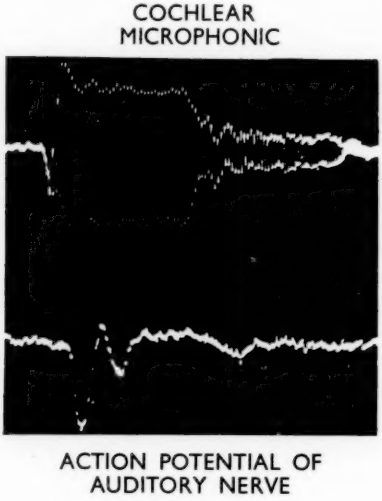

Fig. 9 (*c*)

Fig. 9.—(*a*) Diagrammatic section of the guinea-pig cochlea showing position of insertion of small-diameter wire electrodes into scala vestibuli and scala tympani of the first turn, according to the technique of Tasaki, Davis and Legouix, for the recording of cochlear potentials.

(*b*) Stimulus by a short tone ('tone pip') at 7000 cycles/second, delivered by loudspeaker to the guinea-pig ear.

(*c*) The response of turn 1 of the cochlea to the above stimulus. The cochlear microphonic resembles the sound stimulus, while the response of the auditory nerve consists of a single simultaneous action potential in a number of nerve fibres, followed by a second similar response by a lesser number of fibres.

various situations, as we have noted, because some tissues, such as Reissners' membrane, which are barriers to direct current can transmit alternating currents capacitatively. The cochlear microphonic follows the sound pressure variations with great accuracy, being linearly related in voltage to the sound pressure values, up to a certain sound pressure level, after which further increases of sound pressure produce diminishing increases of cochlear microphonic potential. The response finally reaches a maximum and thereafter diminishes. High intensities, particularly at low frequencies, will cause distortions of the electrical response. There is no threshold, as in neural responses, and the characteristics are not, as in the latter case, all-or-none in character. They also show no sign of latent period, as found in nerve activity.

The cochlear microphonic is now attributed to the external group of hair cells, responding mainly to 'vertical' displacements resulting from the vibrations of the basilar membrane and associated structures. As in the case of the endocochlear potential, 'outward' bending of the hairs causes decreased positivity of scala media.

Stimulation of the endings of the auditory nerve

We may assume, in the light of the foregoing facts, that the store of electrical energy of the endocochlear potential is used as a sort of biological amplifier. Minute sound energy can thus be used to trigger off the energy stored across the face of each cell in the resting potential difference, so helping to explain the extreme sensitivity of the ear. The further events following the cochlear microphonic oscillations are open to some speculation, but one fact is established beyond doubt. The negative phase of each oscillation of the cochlear microphonic (indicating a flow of electric current from scala media to scala tympani) is followed by the occurrence of nerve impulses in the fibres of the auditory nerve. A possible basis for these events was first suggested at the British Association meeting in Belfast in 1952 by Dr. Hallowell Davis, and depends on the suggestion that the hair-bearing ends of the hair cells may act, as has been mentioned above, as a variable resistance across the potential difference between the endolymphatic space and the relatively negative interior of the cells and surrounding tissues. Increased current flow from scala media to scala tympani would traverse the hair cells and nerve fibres, and subsequently pass into the surrounding areas and would thus be capable of stimulating the nerve fibres to produce nerve impulses. Experimental changes of the positivity of the scala media by Davis and his colleagues have given the expected result, i.e. greater current flow from scala media gives greater sensitivity of the ear, and vice versa.

Summating potentials

Associated with the records of cochlear microphonic are changes of the baseline which are due to longer duration, steady shifts in mean endocochlear potential. These are evoked by sound pressure waves in the higher range of intensity. The records show either an upward deflection, indicating increased mean positivity of scala media to scala tympani, and called the positive summating potential, or the negative summating potential, indicating decreased positivity of scala media. These electrical responses are not yet fully understood, but it seems probable that the negative summating potential originates from longitudinal displacement of the internal hair cells, which occurs with high levels of sound, so stimulating their nerve endings by deflection of the hairs towards the apex of the cochlea. The presumption here is that the continuous current which must flow towards scala tympani, and of which the negative summating potential is the visible sign, can generate nerve impulses from the internal hair cells. Conversely, the positive summating potential may be a mechanism limiting the effectiveness of the cochlear microphonic as a stimulus to the nerve fibres at high sound intensities. The summating potential mechanism thus might be a device to extend the dynamic range of the ear, i.e. its ability to handle weak or intense sound stimuli.

Coding of information in the auditory nerve

We have already noted that the cochlear microphonic potential produced by sound stimulation at various parts of the cochlea can be recorded, as has been done by Davis and his colleagues, by using paired electrodes inserted into the scala vestibuli and scala tympani of the various turns of the cochlea of the anaesthetised guinea-pig. Examined in this way, the cochlear microphonics occur in a

manner predictable from the known pattern of vibration of the cochlear partition; within limits the maximum 'vertical' displacement of the basilar membrane and associated structures produces the maximum cochlear microphonic; a necessary qualification being that the magnitude of the microphonic is relatively greater with respect to the vibrational amplitude of the partition, in the basal part of the cochlea.

Action potentials in the auditory nerve

The usual response of a sensory end organ to graded stimuli is that increased intensity of stimulus is signalled by increased frequency of discharge of nerve impulses in the associated nerve fibre. In the case of the ear, this relationship, with some modifications characteristic of the situation, is also seen. Thus the greater the sound pressure, the greater, within limits, will be the cochlear microphonic and the higher the frequency of discharge of action potentials in any appropriate nerve fibre. The discharge of a single impulse in each of a large number of nerve fibres can theoretically result from each negative phase of the cochlea microphonic. This, we believe, happens at the lowest frequencies, but as the frequencies are increased, the nerve fibres cannot respond to these frequencies, and they respond at a lesser rate than the frequency of the sound. The actual rate of discharge in these circumstances is still determined by the magnitude of the sound pressure and in consequence the voltage of the cochlear microphonic.

Superimposed on this cochlear microphonic mechanism, which is associated with the external hair cells, there is the imperfectly understood summating potential mechanism. This can be regarded as we have seen, as a longer-term, non-oscillatory biasing of the endocochlear potential, perhaps capable of modifying the cochlear microphonic, or of itself capable of generating a series of nerve impulses, and responsible for responses to the higher ranges of sound intensity. It also may be associated particularly with the sensation of pitch; the internal hair cells, from their probable lesser sensitivity, finer innervation and association with the summating potential, seem to act as receptors for higher levels of sound and for fine frequency discrimination.

In this respect we can discern some resemblance between the duality of function in the retina, with its rods and cones, and the apparent division of function between the external and the internal hair cells of the cochlea. The former, like the retinal rods, are probably specialised for extreme sensitivity without too great frequency resolving powers; the internal hair cells, like the retinal cones, appear to work at higher stimulus levels, and are apparently specialised for frequency analysis.

We are now able to discern the general principles on which the cochlea acts as the end organ of hearing, and the processes may now be summarised.

The sound waves enter the external ear, set in motion the tympanic membrane and middle ear ossicles, culminating in the movement of the fluid in scala vestibuli, media and tympani. The characteristics of different sensitivity to different frequencies are largely determined by the physical properties of the external ear canal and middle ear, and the transfer of energy to the fluids of the inner ear is extremely efficient.

The vibrations of the fluids set up complex waves in the cochlear partition, travelling from base to apex with both transverse and longitudinal bending of the basilar membrane, organ of Corti and tectorial membrane. The membrane at the basal end follows the waveform of the vibrations, but each frequency component in the wave reaches a rather flat maximum, slows down and disappears at a position on the cochlear partition characteristic of that frequency. High frequencies die out first, low last; with the lowest frequencies, the whole cochlea vibrates.

These vibrational patterns are changed into nerve impulses, probably by means of the electrical currents which appear to be the basis of the cochlear microphonic and summating potentials, so that an amplifying action occurs between the vibrational activity of the cochlear partition and the initiation of nerve impulses. The latter carry the information on frequency of the sound in two ways; first by a direct frequency-following, where one sound wave may initiate one nerve impulse, or a group of nerve fibres may co-operate in producing this frequency-following by responding, in turn, for example, to every alternate sound wave. This method may operate up to 2000 cycles per second or more. The second method is that used at higher frequencies; here the frequency is indicated by the pattern of activity in the cochlea, that is, by a place

identification. This information is, of course, carried by the particular nerve fibre, or group of nerve fibres, originating from a particular part of the cochlea. The significant place on the cochlea is that at which the travelling wave is maximal, or perhaps the place of the rapid loss of amplitude. Sound pressure is indicated by either the numbers of active nerve fibres, or, particularly at the higher frequencies, by the frequency of discharge on individual fibres.

The powers of discrimination for frequency by the mechanical analysis of the cochlea are obviously inadequate to explain the ear's ability to discriminate pitch. It is probable that further sharpening of the selectivity of the system is achieved during the course of the journey through the nervous system, which culminates, presumably, in the arrival of the impulses at the appropriate area of the cerebral cortex, but that is outside the scope of this review.

2. THE EFFECT OF INTENSE SOUND ON THE EAR

The exquisitely delicate and complex structure of the ear is almost continually in motion. Sudden intense sounds, such as that of firearms, may agitate the structures violently. In other cases, a noisy occupational environment may maintain the structures in extreme activity for many hours a day, for a whole working lifetime. It is opportune to enquire what are the effects of such stimulation on these structures and whether there is any relation between hearing ability and the sounds to which the ear is commonly exposed. In addition, it is of interest to enquire whether damage, due to sound, should it occur, affects particularly any specific part of the varied and delicate structures of the ear.

It is, in fact, true that intense brief sounds as from gunfire, as well as long continued high levels of sound, do produce lasting reductions of the acuity of hearing. For such evidence, we may cite references of an historical nature; C. H. Parry (1755-1822) described partial deafness due to noise, and the condition of boilermaker's deafness has been widely recognised since the early days of heavy industry.

Before proceeding any further, certain definitions will assist further discussion. Sound which is intense, possibly unpleasant and unwanted is known as *noise*. *Noise exposure* implies the physical stimulus to the ear; it may be described in terms of the physical characteristics of the noise, in conjunction with the duration of exposure. The physical characteristics of the noise, as we have seen, have many dimensions, in terms of intensity, frequency distribution and time-pattern, so that the specification of exposure is not an easy matter. *Hearing loss* is a term implying some definable degree of reduction in auditory acuity compared with some specified standard, or previous condition. It does not imply total loss of hearing. Noise may cause reductions of acuity of hearing which may be temporary or permanent. These reductions in acuity can be quantitively measured, in the way which Dr. Littler will be describing later today, and may be classified according to their origin into three categories.

Temporary threshold shift

This is a temporary reduction of hearing acuity. Its name implies that the auditory threshold, that is the weakest sound which is just audible at any particular frequency, is somewhat increased. Temporary threshold shift follows almost any exposure to sound, and spontaneously disappears in minutes, hours or days, if the exposure has not exceeded some critical value. The latter is nearly impossible to define precisely, due to the potential variability of the stimulus and the individual variability in susceptibility to damage in different ears. Temporary threshold shift is characterised by a reduced sensitivity of the ears to frequencies of sound somewhat higher than the sound which caused it, if the latter is a pure tone. Even if the sound which caused the effect is spread over a band of frequencies the temporary threshold shift will still show this upward tendency. It occurs most in the first few hours of a working day, and it is most marked in young, normal ears, and least in ears already impaired by exposure to noise. It has been studied experimentally in man.

Acoustic trauma

This term denotes permanent hearing loss of some defined degree produced by very intense sound, acting for a short time. It may include damage to the tympanic membrane and middle ear, as well as to the cochlear structures. This type of

injury may also be found after impact injuries to the head, and obviously cannot be studied in man except in the case of accidental exposures or injuries. It has been studied experimentally in animals, by using the ears of anaesthetised guinea-pigs. Records of the cochlear potentials before and after intense sounds, with microscopic examination of the tissues of the cochlea, have shown various degrees of injury. These range from slight, and probably temporary changes in the hair cells, up to massive and permanent damage in these cells, or even disruption of the organ of Corti. Parallel changes occur in the size and characteristics of the cochlear microphonics. It is likely that the ears of the guinea-pig and man are very similar in their response to grossly damaging sound, and that such changes would also occur in human ears with similar damaging exposure.

Occupational hearing loss

This is a very wide category and in fact it is not always easy to draw a clear line between 'normal' ears and ears with some hearing loss. It has been tacitly assumed in the foregoing that there are 'normal' and 'abnormal' ears. The difficulty of precise definition of the 'normal' category will be emphasised by Dr. Littler in his paper on the procedures and results of making audiometric measurements of the hearing of various groups of people. The facts are that, in the conditions of life today, even without exposure to appreciable occupational noise, our ears appear to sustain enough constant noise exposure to produce, over a number of years, some unnoticed, but still measurable losses in excess of the normal deterioration which is inevitable as age advances, and which would be found in people with minimal exposure to noise.

However, some occupational noises do exist which, in a majority of people, produce effects which are easily detectable by the technique of audiometry, if the ears have been exposed daily for periods of perhaps years, in the course of work. This type of hearing loss is insidious, inasmuch as the onset is slow and unnoticed. Some ringing in the ears and dullness of hearing may be noticed at the end of a day's work, especially in the earlier stages, but the noise may not give any offence, or appear excessive. The consequences for the ear have no specific relation to annoyance, which is altogether another matter, and may occur at any level of intensity. This aspect will be dealt with specifically by Mr. Broadbent later today.

One fortunate feature of occupational hearing loss is that, in the early stages, it nearly always results in reduction of acuity, perhaps even of a temporary nature, for tones of a rather high pitch, often in the region of 4000 cycles per second. At this stage the degree of loss which can be identified by audiometric tests, especially if the hearing has been measured before starting work in a definite noisy occupation, is very small, and is completely impossible to recognise subjectively. This warning sign is reliable and definite, and provided audiometric tests are periodically made, incipient occupational hearing loss need not pass undetected.

However, if further exposure occurs, the condition will progress. Deterioration of hearing then becomes more marked at the higher frequencies and spreads downwards to involve lower frequencies, so that instead of localised loss in the region of 4000 cycles per second, frequencies below this begin to be affected. For the perception of speech we require the frequencies from 300 to 3000 cycles per second as a minimum, so that significant deterioration in this region will become noticeable as an inability adequately to hear conversation. The lower the frequency which it seriously affects the greater will be the disability, so that finally an obvious social handicap exists.

Only in certain people does the degree of exposure combined with the degree of individual susceptibility to aural damage produce an obvious disability; the noise levels need not be extreme; if they are, discomfort or actual pain in the ears demands protection. In the latter case, precautions are normally taken and aural damage is prevented. It is in the intermediate range, where people are unwilling to go to the trouble of wearing ear protection, that appreciable occupational hearing loss may occur.

Relations between temporary threshold shift, occupational hearing loss and acoustic trauma

It is of importance both academically and practically, to understand the physiological or pathological relations between these three effects. Where gross disruptive damage is done to the organ of Corti and associated structures in acoustic trauma, this is obvious on histological examination. However, the more subtle relationships between tem-

porary threshold shift and occupational hearing loss present far greater problems. It is agreed that these effects are basically peripheral, and due to some changes, temporary or permanent, in the cochlea. What these changes are we do not yet know. It is possible that microchemical and electron microscopy techniques may shed light on the essential nature of these conditions. At present it is being widely assumed, in the absence of conclusive information, that temporary threshold shift effects may be used as an indicator of the probable characteristics of occupational hearing loss. This may or may not prove to be justified, but definite information on these interrelations is one of the more urgent requirements for a better approach to the practice of hearing conservation.

Prevention of occupational hearing loss

The relations between noise exposure and hearing loss are exceedingly complex, and in serious need of large-scale investigation. Work is going on both in the U.S.A. and in Britain, but not on a sufficient scale, at least in Britain, to yield data in adequate quantities in a reasonably short time. It is to be hoped that ways may be found to remedy this deficiency.

At present however, sufficient is known to provide a reasonable approach to the conservation of hearing. Stated very briefly, the principles are as follows. First, the noise must be measured in terms of intensity, distribution of energy throughout the frequency spectrum (a single measure of overall intensity is not adequate in most cases), and the time pattern. The duration of exposure of each person or class of persons should be estimated. Consideration of these two quantities, noise and duration, will enable the potential harmful effects of the noise to be assessed against some standard or criterion of potential harmfulness. Such a criterion* is shown in Table 1.

TABLE 1

MINIMUM SOUND PRESSURE LEVEL VALUES, IN SPECIFIC FREQUENCY BANDS, AT WHICH HEARING CONSERVATION SHOULD BE INSTITUTED

Frequency band (cycles per second)	*Sound Pressure Level* (decibels)
37·5–150	100
150–300	90
300–600	85
600–1200	85
1200–2400	80
2400–4800	80

If the exposure is judged to be harmless, it may be decided that no further steps need be taken. If it is marginal, individual judgment must be employed in the handling of the situation. If it is clearly harmful to hearing, conservation measures must be taken. These will include (*a*) reduction of the noise if this is practicable; (*b*) if not, reduction of the exposure by the wearing of ear protection, segregation of people away from noise, or reduction of working hours in noise, perhaps by rotation of jobs or similar devices; (*c*) pre-employment audiometric examinations of hearing, followed by periodic audiometry to ensure that the precautions are effective and to protect those with abnormally susceptible ears. By these means the accidental acquisition of occupational hearing losses can virtually be eliminated.

In this brief sketch of the problem of noise from the viewpoint of hearing loss, many significant aspects have necessarily been omitted. Further research on the relations between noise and hearing loss would remove many uncertainties and deficiencies in our knowledge, and make the working of hearing conservation easier. As it is, there is no substitute for individual audiometry, which has a key position in any conservation scheme. Hearing loss is a disability which can and should be avoided, in order to avoid inconvenience and retain the pleasures of adequate perception of speech and music. It is perhaps worthy of note that the vast majority of noises which give annoyance are completely incapable of producing any significant alteration in our hearing, and must not be judged in this light. The problem of annoyance is a difficult one, is virtually different for each person, and what may annoy one individual may delight another.

This sketch of the working of the ear has attempted to show its complication, its elegance and refinement of function, as well as our shortcomings of knowledge of its complexities. These attributes, as well as its extreme sensitivity, its tremendous dynamic range, and its relative robustness in action,

* These values only apply to broad-band, steady noise, to which persons are exposed for the whole working day, five days per week. The sound pressure levels may be exceeded if the exposure duration is reduced, but the relationships are only approximately known.

make it a source of wonder, perplexity and admiration to those who study it, and will no doubt continue to do so for many years to come.

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LOOKING AT science

Biology films at Cardiff

Fifty films were shown at the Reardon Smith Lecture Theatre of the National Museum of Wales—seven in a special programme devoted to World Food and Population (designed to provide background information for members attending the highly successful Symposium on the Monday); twenty-nine in two general programmes (each of which was repeated once); and the remainder in two programmes concerned with the use of the cine camera in research (presented by the Technical and Scientific Group of the Royal Photographic Society).

These programmes attracted the best attendance recorded at any of this series since it was started at the Bristol Meeting in 1955, and on at least one occasion members were seated in the aisles for lack of seats (in a theatre seating over 450), whilst others queued in the rain outside.

We publish below details of the biology films included in these programmes (with the exception of some shown in the Saturday morning research programme). These films may be obtained from the distributors indicated, to whom all enquiries should be sent.

Animals in Spring 11 MINUTES U.S.A. 1954

Produced by the University of Chicago. Available on hire from the G.B. Film Library, Aintree Road, Perivale, Greenford, Middlesex.

One of a series of four films on animal behaviour throughout the seasons of the year. This film includes examples of: Spring peeper frog, turtle, salamander, American robin, wild ducks, quail, rabbits, lunar moth, caterpillar, flying squirrel, screech owl, mentor, mallard, woodchuck, fox, water beetle larvae and sunfish.

Faune Cavernicole 20 MINUTES FRANCE

Produced by Filmtec. Available free from the French Institute, Queensberry Place, London, S.W.7.

An unusual laboratory established in the caves at Moulis. This is devoted to a study of animal life in caves, ranging from primitive organisms to varieties of familiar creatures like lice which are specially adapted to life in permanent total darkness.

Human Heredity 15 MINUTES U.S.A. 1956

Produced by Churchill-Wexler Film Productions for the E. C. Brown Trust of Oregon. Available on hire from Boulton-Hawker Films, Hadleigh, Suffolk.

Intended for use with classes which already understand the basic facts of human reproduction and growth. This American teaching film shows, largely with animation, the principles of sex development and the transmission of hereditary characteristics.

The Life of Plasma cultivated in the Laboratory $9\frac{1}{2}$ MINUTES FRANCE

Produced by the Institute of Scientific Research and available on free loan from the French Institute, Queensberry Place, London, S.W.7.

Time lapse studies on the growth of plasma.

The Living Soil 20 MINUTES GREAT BRITAIN 1960

Produced by the Shell Film Unit. Available from Shell International Petroleum Company, No. 1 Kingsway, London, W.C.2.

An account of the fight against pests in the soil. This film includes a study of the research into more effective methods of controlling these pests and the application of these methods.

Micro-organisms—Harmful Activities

15 MINUTES U.S.A. 1958

*Produced by the Audio-Visual Centre, Indiana University. Available on hire from Gateway Film Productions, 470 Green Lanes, Palmers Green, London, N.*13.

An outline study which demonstrates Koch's postulates: (*a*) Observe the same organism in every case of the disease, (*b*) Isolate the organism and grow as a pure culture, (*c*) Inoculate the pure culture into a susceptible host to reproduce the disease, and (*d*) Re-isolate the same organism from the experimentally diseased host.

Mitosis 9 MINUTES AUSTRALIA 1958

*Produced by the Commonwealth Scientific and Industrial Research Organisation. Available free from the Australian News and Information Bureau, Australia House, Strand, London, W.C.*2.

An account, mainly in animation, of the principles of cell division.

Mountain Lands in Jeopardy

14 MINUTES NEW ZEALAND

*Produced by the New Zealand Film Unit. Available free from the New Zealand Film Library, Adelphi, John Adam Street, London, W.C.*2.

The former 'delicate balance between vegetation and erosion' which existed in the New Zealand Alps was upset when deer, goats and other animals were imported to an area with no natural enemies. As a result soil erosion has become a major problem and the film shows some attempts to solve it (by prohibiting fires in forest areas and shooting the pests at the rate of 135,000 a year).

Spruce Bog: An essay in Ecology

23 MINUTES CANADA 1959

*Produced by the National Film Board of Canada. Available on hire from the Central Film Library, Bromyard Avenue, London, W.*3.

A remarkable visual study of the development of a spruce bog into forest. Natural or artificial damming will produce an area of still water which is gradually invaded by sphagnum moss, from which the bog develops.

Unseen Enemies

32 MINUTES GREAT BRITAIN 1960

*Produced by the Shell Film Unit. Available free from Shell International Petroleum Company, No. 1 Kingsway, London, W.C.*2.

A report on some problems of communicable diseases, such as leprosy, yaws and malaria. The film emphasises that medical provisions in themselves are not enough and that there are many social problems to be tackled if the health of the human race is significantly to be improved.

BOOKS

The Biology of Marine Animals. By J. A. Colin Nicol. 707 pp. (London: Sir Isaac Pitman & Sons Ltd.) 95*s*.

Dr. Colin Nicol's book is essentially one for the specialist in marine zoology, for in no sense can it be regarded as a popular exposition of the subject. Indeed, he makes it plain in his preface that he is writing for the 'undergraduate specialising in marine zoology' and also for senior members of universities. There can be little doubt that this is the most authoritative book so far on the physiology of marine animals and as such will prove to be an essential part of the equipment of all serious students of marine zoology.

Dr. Nicol is a physiologist, and since representatives of nearly every major group of the animal kingdom are found in the sea, there is a good deal of overlap with books dealing with comparative physiology as such. This is not to say that Dr. Nicol does not bring a fresh and authoritative approach to many topics. He does! But, in writing for the specialist, he quite rightly assumes a basic knowledge of many aspects of zoology in his readers and those who lack this will find much of his writing difficult to understand. Specialised though it is, the book should find a welcome not only among marine zoologists but among all students reading for an honours degree in zoology. If only for reference, it deserves a place in all biological sixth-form libraries—despite its somewhat formidable price.

The illustrations are numerous and clear; the format excellent and the bibliographies at the end of each of the fifteen chapters full and up-to-date. No book, however good, is perfect but there are fewer than usual of the inevitable misprints and factual errors. It would do no service to enumerate these since they concern specialists only and, being of a minor nature, in no way detract from the merit of the book as a whole.

G.E.N.

Science Survey. Edited by A. W. Haslett and John St. John. (Vista Books.) 25*s*.

The right kind of popularisation of science is today both more necessary and more difficult than it has ever been. It is more necessary for the obvious reason that science is having an ever greater effect on the material framework of our lives, on our social and economic policies and on our ideas. It is more difficult because the rate of growth of science is so rapid that the frontiers of discovery have become alarmingly remote from the basic knowledge which the ordinary intelligent man can be assumed to possess, even on the most optimistic supposition. Further, it is not only the non-scientist who now needs popular accounts of recent discoveries; it is the scientist working in other fields. 'Science' is no longer a body of knowledge with the whole of which a single mind can be reasonably familiar, still less competent. Even the graduate in, say, chemistry, will often be almost as ignorant as the layman about recent advances in nuclear physics or genetics. Gulfs of incomprehension yawn not only between scientists and non-scientists, but between different kinds of scientist. *Science Survey* is an unusually successful attempt to bridge some of those gulfs.

It is an excellent piece of work in two ways. First, the topics discussed have clearly been most carefully chosen. They succeed in giving a representative picture of the lines of research which are now most active and most promising without losing sight of what the educated reader will want to hear about. The seven major headings under which the contributions are grouped include such topics as fundamental particles, large molecules and their uses, cells and viruses, animal behaviour, and the exploration of space. Every scientific worker will think of subjects which in his view should have been included, and which he hopes will have priority in future volumes. But the most partisan will not deny that the actual selection gives a fair picture of the range of scientific work and of the kind of problem which is being most vigorously tackled.

The second feature that deserves the warmest commendation is the choice of authors. These are all people who are themselves making active contributions to the subjects about which they write. The danger of such a choice is clearly that such authors will be so involved in their particular work and so remote from the non-specialist reader that they will too easily become over-detailed and incomprehensible, and quite clearly this has happened in a few of the contributions. But on the whole this danger is more than compensated by the advantage that the expert has in writing about his own subject, the advantage of enthusiasm. Even if through lack of background one is baffled by some of the chapters, at any rate one realises that something exciting and significant is being done.

Excellent though the book is, there is no doubt that some who are interested in the scientific education of the non-scientist will criticise it for two reasons. The first is on grounds of difficulty. And here is must be admitted that it will probably be read more by science specialists anxious to know a little of the work in other fields, than by the real layman. There are, indeed, very few non-scientists who could begin to understand the contributions on cells and viruses, or those on 'arranging atoms for industry', although Prof. Frisch on fundamental particles and the chapters on the Earth manage to be astonishingly lucid. The fact that so much of a book of this kind will daunt an intelligent non-scientist is, it may well be contended, a criticism of the science that we teach (or fail to teach) in our schools.

The second possible criticism is more fundamental. There is a school of thought that maintains that it is essentially unscientific to give non-scientists information about recent work which they but half understand and which they are in no position to evaluate. The logical conclusion of this attitude is that the non-scientist should learn some very limited aspect of science intensively, so that he may understand 'how a scientist's mind works'. There is something to be said for this line of thought, but it is not incompatible with a simultaneous attempt to indicate the direction in which advance is now proceeding. One of our aims in scientific popularisation must be to communicate not simply knowledge but a sense of excitement in discovery. The editors of this book are to be congratulated on their measure of success in achieving that difficult task.

JAMES OF RUSHOLME.

Geochemistry for Everyone. By A. Fersman (trans. A. Myshne). (Foreign Languages Publishing House, Moscow, 1958.) 454 pp. 12*s.* 6*d.*

Alexander Fersman was one of the most distinguished Russian mineralogists and geochemists of this century. This volume, published originally in 1948, five years after his death, was completed by his colleagues from notes which he left.

In these days of the rapid fragmentation of science generally, one tends to accept unquestionably the modern inventions as part of everyday life, and to give little thought to the sources of the materials which make their development possible. This is in part due to the common failing of scientists in speaking a language which is unintelligible to any but the expert. Fersman made a point of writing several books which were aimed at reaching the layman and thereby stimulating interest in his subject. In this, his last work, he achieves a conspicuous success, which through the admirable translation is now available for English readers at the ridiculously low price of 12*s.* 6*d.*

Fersman feels quite rightly that only good can come from encouraging people to read about the planet they live in, and about its economic resources. The book is full of homely analogies, anecdotes and reminiscences which serve to bring home the salient points the author is making. The author discusses the structure of the atom and then deals at some length with the important discoveries of Mendeleyev. Emphasis throughout is naturally given to Russian work, but it is good to see that credit is given to others where it is due, e.g. Davy, Kelvin, Rutherford. The main section deals with the sources of the main economic raw materials and considers how and why they have become so concentrated. Then finally the author may be forgiven for a fanciful flight into the future by giving his views of how nature will be harnessed and profitably used.

The book is excellently produced, and in particular, note is made of the fine binding. Although abundantly illustrated the photographic reproduction is poor. The page margins are filled with frequently amusing caricatures which emphasise some point in the text. The book will be enjoyed by all those who wish to know more of the planet in which they live and who, like the reviewer, are curious about the Russian scene.

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